JOURNAL

OF

THE FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

JUNE, 1864.

CIVIL ENGINEERING.

For the Journal of the Franklin Institute.

The Arch Truss Girder again.—More upon the New System.

By S. W. Robinson, C.E., Detroit, Michigan.

My article in the March Number of your Journal, contained in its latter portion the statement, *without the proof*—of a few principles and formulæ as applied to my system of the "*Arch Truss Girder*," where the curve of the arched chord is such,—the ties all being normal to it,—that the stress is uniform throughout the cord for a uniform horizontal loading throughout. The stress of the diagonals in the case there considered was compression.

That may be well where the girder is executed in wood; but if iron be employed it may be desirable that the diagonals resist tension. The origin was also taken at the crown.

I have found it more convenient to reckon the number of the pannel, or bay from one end.

In this article I propose to consider the case where the diagonals suffer tension; and to give rigorous analytical proof of every principle there stated, or here employed: and also, to reckon the number of pannel from the end of the truss.

Dimensions of the Parts.

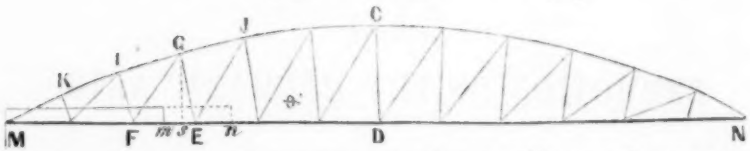
The formulæ given before would apply here; the only modification

necessary being that required for reckoning the number of bay from the end.

Using the same notation as before, and taking the origin of co-ordinates at the point M in the Figure, we have

$x' = \text{B} - x$, and $y' = a - y$; or $x = \text{B} - x'$, and $y = a - y'$; which substituted in the expressions given before for the co-ordinates of K, I, G, &c., in the Figure, and dropping the accents give

$$\left.\begin{aligned} y_n &= \cos.\ i_n \left\{ a - \frac{\text{B}^2}{4a} \sin.^2 i_n \right\} \\ x_n &= \text{B} - \sin.\ i_n \left\{ \frac{\text{B}^2}{2a} + a - \frac{\text{B}^2}{4a} \sin.^2 i_n \right\} \end{aligned}\right\} \quad (1)$$



When FIGE is the n^{th} pannel $n = 3$; the co-ordinates of G are x_3 and y_3; GE $= l_3$, GF $= \text{L}_3$, GI $= \text{C}_3$, ME $= b_3$,

$\text{M}m = b'_3 = \dfrac{b_3 + b_2}{2} = \dfrac{b_n + b_{n-1}}{2} = b'_n$ and the angle at G $= i_3$ the angle of GI with ME $= i'_3$.

The angle of $i_\circ$ at M is given by the formula.

$$b = \frac{\text{B}^2}{2a} \sin.\ i. \quad . \quad . \quad . \quad . \quad (2)$$

of the last article by making $b = \text{B}$.

$$\therefore \quad \sin.\ i_\circ = \frac{2a}{\text{B}}, \quad . \quad . \quad . \quad (3)$$

Hence for any angle i_n we have

$$i_n = i_\circ - \frac{n i_\circ}{\text{N}} = \frac{i_\circ}{\text{N}} (\text{N} - n). \quad . \quad . \quad (4)$$

Equation (2) gives the distance from the middle to any point.

$\therefore\ b_1 = \text{B} - b$ or

$$b_n = \text{B} - \frac{\text{B}^2}{2a} \sin.\ i_n \quad . \quad . \quad . \quad (5)$$

for the distances MF, ME, &c.

The expression for l as before is

$$l_n = a - \frac{\text{B}^2}{4a} \sin.^2 i_n. \quad . \quad . \quad . \quad (6)$$

The length of the diagonal FG is

$$\text{L}_n = \frac{y_n}{\sin.\ \theta_n} = \left((x_n - b_{n-1})^2 + y_n{}^2 \right)^{\frac{1}{2}} \quad . \quad . \quad (7)$$

in which tang. $\theta_n = \dfrac{y_n}{x_n - b_{n-1}}$. . . . (8)

The length of any part of the arched chord is

$$c_n = (x_n - x_{n-1}) \sec. i'_n,$$

in which $i'_n = \frac{i_n + i_{n-1}}{2}$.

Stress of the Parts.

The various parts of a truss built for any purpose, should be so proportioned as to withstand the greatest strain ever liable to come upon them. It is for this maximum stress that I have designed these formulæ for the truss.

If no other except a uniform loading throughout should ever come upon the arch truss, no diagonals would be necessary, nor would any compression ever occur upon the normals. This is the case theoretically, with the roof, but practically it is not. Snow and ice are liable to gather upon one side. This case would be represented more approximately by a load over part of it. And applied to railroad bridging the strength of each part is severely tested. Here the load may be regarded as uniform; and a long train moving over extends from the end upon which it enters, at different instances, to various parts along the roadway until we have the case of a uniform load throughout.

The stress of the diagonals being zero before the load enters, and zero after it entirely overreaches the bridge, and not being zero for a partial load, it is plain that each diagonal has a maximum as this load passes along.

I shall attempt to prove that the diagonal has its maximum as the end of the load reaches the middle of the bay to which the diagonal considered belongs: and that the chords have their maximum when the load extends entirely over.

Throughout this article in speaking of a loading, the uniform load as far as it extends is always referred to.

Now let the maximum stress of the upper or lower chord, at any points, as G or s, be required. Pass a plain through G, perpendicular to the straight chord. The two parts of the truss GEN and GFM being perfectly ridged may be regarded as single pieces. Then, according to principles demonstrated in "mechanics," there will exist in the upper chord a horizontal force equal and opposite to that in the lower chord, (since all the external forces are vertical) which will be the sum of the moments of the external forces taken upon either side of this section.

If the load extend, as represented in the Figure, a distance x beyond this section the sum of moments will be.

$$(Bw + V)\, Ns - Wx\frac{x}{2} = H\, Gs.$$

Also by moments

$$V \,.\, 2B = W\,(Ms + x)\frac{Ms + x}{2},$$

in which W = the weight per unit of the load,

w = the weight per unit of the truss,

and V = that portion of the vertical force at N resulting from the partial load; which substituted gives

$$H = \left(B w + \frac{W}{4B}(Ms + x)^2\right)(2B - Ms)\frac{1}{Gs} - \frac{W x^2}{2Gs} = \max. \qquad (10)$$

Placing the first differential co-efficient equal zero the first condition for a maximum gives $x = 2B - Ms$.

The second differential co-efficient $= -Ms$, which satisfies the second condition; that is, for a maximum of either chord the load must extend entirely over the truss. Hence

For the maximum strain of the upper chord we will have the expression $W\frac{B^2}{2a}$ or,

$$T = (W + w)\frac{B^2}{2a} \qquad (11)$$

This is also equal the maximum of the straight chord at the middle, but the ties diminish it toward the ends as given in the last article.

Hence since $b'_1 = B - b'$ we will have for

Maximum strain of the straight chord

$$T = \frac{(W + w)}{2a}\left(B^4 - 4a^2(B - b')^2\right)^{\frac{1}{2}}. \qquad (12)$$

Tensile strain of the normals.

It is plain that the weight of the truss affects only the chords and normals, since from a uniform load there results no strain upon the diagonals.

The total tensile strain upon the normals from the middle to one end was found to be $P = \frac{wB^2}{2a}\ i_0$.

Hence the strain upon each for this case will be

$$\frac{P}{N} = (W + f w)\frac{B^2}{2Na}\ i_0 = p, \qquad (13)$$

in which f is a fraction expressing what part of the weight of the structure is contained in the roadway and straight chords. The value of p will be at its maximum when the load is entirely throughout, for the stress of the diagonals, being tension, tends toward diminishing that of the normals.

Stress of the diagonals.

The stress of the diagonals each have their maximum as the load in passing over, arrives at the middle of the bay considered. This strain being a function of the horizontal and vertical forces will have its maximum when the sum of the components of these forces in the diagonal have their maximum.

Let t_1 and t_2 represent these two components resulting from H and V respectively, h_1, h_2 the components in the chord.

Then, (referring to the figure,) the maximum of GF is for the load M m, upon the hypothesis assumed. If it be otherwise, let the load extend beyond m, a distance x. The expression for H is given in (10) by making $Ms = Mm = b'$, and neglecting w. This has its maximum for a load throughout, as has been shown. But the vertical force at the point considered is equal the vertical force at N, diminished by the opposite vertical force resulting from the load between m and M, or the transverse shearing at m.

Hence we will have

$$V = \frac{W}{4B}(b' + x)^2 - Wx = \frac{b'}{4B} + \frac{x}{4B}\Big(x - 2(2B - b)\Big). \qquad (14)$$

which has its maximum for $x = 0$, V being at its maximum for a load only to the point considered, the sum of the components from V and H may have their maximum at this point.

Resolving the forces in the diagonal GF, and the part of the chord GI which together resist H and V we get,

$$h_1 \cos. i'_n - t_1 \cos. \theta_n = H_n, \; h_1 \sin. i'_n = t_1 \sin. \theta \;,$$
$$\text{and} \; -h_2 \sin. i'_n + t_2 \sin. \theta_n = V_n, \; h_2 \cos. i'_n = t_2 \cos. \theta_n,$$

from which

$$t_1 = \frac{H \text{ tang. } i'}{\sin. \theta - \cos. \theta \text{ tang. } i'}, \text{ and } t_2 = \frac{V}{\sin. \theta - \cos. \theta \text{ tang. } i'}. \qquad (15)$$

For a maximum of $t = t^1 + t_2$ we may omit the common denominator, and the expression that will give the maximum, will be

$V + H$ tang. i; or, substituting H and V as given above we will have

$$\left\{ \frac{W}{4B\,y}(2B - b')(b + x)^2 - \frac{W x^2}{2y} \right\} \text{ tang. } i + \frac{W}{4B}(b' + x)^2 - Wx = \text{max.}$$

This may be transformed to

$$\frac{b'^2}{4By}\left\{ (2B - b') \text{ tang. } i + y \right\} - \frac{x}{4By}(b' \text{ tang. } i - y)(x - 2(2B - b')) \qquad (16)$$

This will have its maximum when $x = 0$. For x being the only variable in the case, the first term is constant, and in the second term b' tang. $i < y$, and $x < 2B - b'$ which makes the second term negative for any applicable positive values of x. I think no formula is necessary to show that t is no greater for a shorter load than b', since both H and V increase to the section considered;

That is, for a maximum stress of the diagonal, the loading must extend to the middle of the bay considered, and no farther.

The expression (16) does not hold for x negative, which corresponds to a shorter load than b'. For this case the moments must be taken upon the other side of the section. The conditions for a maximum from the differential calculus fail here.

The condition $\frac{dy}{d} = 0 =$ tangent, is satisfied only where an infini-

tessimal element of the arc constructed from $y=f(x)$ is horizontal. In the case above considered where $t=f(x)$ the ordinate t continues to increase from N to the section considered, and beyond. Consequently the tangent does not equal zero at this point. But there are points where it equals zero which would be indicated by the condition $\frac{dy}{dx}=0$.

If the curve of the arched chord have any form whatever, concave toward the straight one; or if both are straight and parallel or not, the expression (16) holds equally true. For the extraordinary case in which the curve is convex toward the straight chord, b' tangent $i>y$, and the load must extend beyond the bay considered.

If the diagonal resist compression, the expression b' tangent $i<y$, is still true: or, the conditions for a maximum of t remain unchanged.

Therefore, the maximum stress of the diagonals for every *practical* case above mentioned, will be

$$t_1+t_2=\mathbf{t}=\frac{\mathrm{V}_n+\mathrm{H}_n \text{ tang. } i'_n}{\sin. \theta_n-\cos. \theta_n \text{ tang. } i'_n} \quad . \quad . \quad (17)$$

The weight of the truss having no effect upon the diagonals, the expression for H and V, to be used in this formula are those resulting from (10) and (14) by neglecting w and making $x=0$. Or

$$\mathrm{V}_n=\frac{\mathrm{W}\, b'^2{}_n}{4\mathrm{B}} \text{ and } \mathrm{H}_n=\frac{2\mathrm{B}-x_n}{y_n}\mathrm{V}. \quad . \quad . \quad (18)$$

Compression of the Normals.

The maximum compression of a normal occurs when the load arrives to the centre of the bay considered. Thus for the normal GE, the load extends to m: for FGJE may be regarded as a pannel, and the case comes under one already discussed above. Hence calling C the required compression and noticing that GE and GF resist H and V, the resolved forces become

$$c_n \cos. i'_n - h \sin. i'_{n+1}=\mathrm{V}_n$$
$$c_n \sin. i'_n + h \cos. i'_{n+1}=\mathrm{H}_n$$

from which

$$\mathrm{C}_n=\frac{\mathrm{V}_n+\mathrm{H}_n \text{ tang. } i'_{n+1}}{\cos. i_n+\sin. i_n \text{ tang. } i'_{n+1}}. \quad . \quad . \quad (19)$$

In all these formulæ the algebraic sign of the trigonometrical function of i must be observed.

These principles, for all kinds of truss bridges have been fully developed, and will probably soon appear before the public in a thorough and excellent work upon "Engineering" about to be issued by De Volson Wood,—Professor of Civil Engineering, University of Michigan.—Reference need only be had to his numerous articles published in our journals to insure all of the original, and genuine character of the work.

By means of a model constructed expressly for the purpose, he has been able to prove the results of his investigation of the truss by actual experiment, and to bring into the clearest light many errors

existing in works now constantly referred to by the "*practical engineer.*"

Comparison of the new, and the ordinary parabolic systems in numerical results.

In order to show more clearly what I have obtained by these investigations upon the truss, I have computed by the preceding formulæ the value of the stresses of some of the principle parts for the two systems, which are tabulated below.

The quantities computed for the arch truss with parabolic arc, are the maximum values according to the same principles by which the new system has been treated.

The stresses of the diagonals acting as either ties or braces have been calculated for both systems. The example I have taken is for $2B = 160$, = span; $a = 20$ = depth of truss; $2N = 12$ = whole number of bays; $w = 0$; $w = 1$; the same for both systems.

From this data I get for the compression of the arched chord throughout for this system, . . 160

The same at the middle of lower chord.

At the end bay of lower chord, . . . 141·965

For parabolic system, arched chord at the crown, . 160

At last bay, ... 178·886

Lower chord throughout, . . . 160

THE DIAGONALS, AS TIES OR BRACES.

	NEW SYSTEM.		PARABOLIC SYSTEM.	
Bay.	as ties.	as braces.	as ties.	as braces.
2	16,480	20,220	19,524	16 500
3	20,222	21,919	20,905	18,078
4	21.111	23,786	22,686	20,488
5	23,247	24,926	23,864	22,502
6	24,615	25,183	23,811	23,766
7	25,098	24,532	23,710	24,185
8	24,650	22,986	22,322	23,673
9	23,245	20,617	20,140	22,302
10	20,893	19,227	17,402	20,125
11	17,686	14,286	14,700	17,395

It appears from these that the greatest stresses upon the diagonals are for those of the new system. The difference of the sums total of the two systems as ties is 8·183, and as braces 8·668. But this distributed among the ten diagonals are the hypothesis that an equal amount belongs to each, gives about 0·8. Or, the mean of the stresses being about 20, the increase of stress is about $\frac{4}{100}$, some more and some less. But at the same time there is a diminution in the total lengths of all the diagonals of about $\frac{1}{400}$.

But there is also an increase of stress in the normals, over the vertical ties of the parabolic form, of about $\frac{1}{20}$: but at the same time there is a gain in the length of about $\frac{1}{48}$ in favor of the new system.

Although the arguments founded upon the diagonals and normals against the new system slightly outweigh those in favor of it, still the great decrease of stress in the arched and straight chords must greatly

overbalance them all. The diminution of stress for the arched chords near the abutments as may be seen above is 18·886, and in the straight chords 18·035, or a saving of about $\frac{1}{9}$ of the stress, or of area of section near the ends of the truss. These quantities vary with a or B.

If we consider the weight of the truss and make w nearly equal W, the amount of stress for the diagonals remains unchanged, *while the saving of stress to the chords is nearly doubled.*

In the "Howe's Truss" the stress of the chords diminish toward the ends. At the same time that of the diagonals is greatly augmented while the length remains the same. Probably this alone is sufficient to condemn the system for iron bridging. I have observed that iron bridges, under the arch truss system with parabolic arc, are quite common. But as iron gains the ascendency over wood as a building material for bridges,—*why is not this system with normal ties destined to become adopted as one of the most common forms?*

In the preceding part of my last article I stated, where integrating for expressions for co-ordinates to the curve, that I had not attempted the complete integration of the expression as a function of x and y only. I have since received a communication containing an algebraic solution of my final equations of $y=f(i)$ and $x=f(i)$ from Mr. E. P. Austin, Astronomer, formerly Assistant Astronomer to U.S. Lake Survey; he proceeded by transferring the origin of co-ordinates to the centre of the curve, transforming the trigonometrical functions into those of the cosine, placing the expressions $=0$, and eliminating by dividing one by the other repeatedly, until i disappeared. He gave the following for the form of the function,

$$a\,(x^2+y^2)^3+(b+cy^2)\;(x^2+y^2)^2+(d+ey^2+fy^4)\;(x^2+y^2)+gy^4+hy^2+i=0,$$

in which the quantities a,b,c, &c., are functions of a and ρ_0.

This, he adds, is an equation of a curve which has a centre, because all the powers of the variables are of an even degree.

ERRATA TO LAST ARTICLE.

For third equation after (17) on p. 155, read $T'=\frac{WB}{2a}\left(1+\frac{4y^2}{x^2}\right)^{\frac{1}{2}}$

Equation (18) on p. 156, for sin. $\frac{-12a}{B}$ read, sin.$^{-1}\frac{2a}{B}$.

Result of Experiments on the Breaking Weight of Rolled Iron.

From the London Builder, No. 1110.

[From the paper by Mr. F. A. Paget, C. E.]

When we remember that the very best wrought iron of commerce is, to use the words of the well-known metallurgist, Saint-Claire Deville, but a metallic sponge, like platinum, the pores of which have been simply closed up by pressure or percussion; that, in one word, ordinary wrought iron has never as wrought iron, been fused, it will be seen that the uncertainties qualifying the material itself are still greater. Mr. Mallet thus found that, while the original hammered

slab of a very large forged mass had a breaking strength of 24 tons to the square inch, it fell progressively to 17 and 16 tons at the different places of the mass, down to even as low as 6½ tons in some parts. Unless this iron had been burnt, its tenacity could doubtlessly have been restored; and, if drawn into wire, its breaking weight might have been increased to perhaps 90 tons to the square inch,—at least before annealing. An average of 188 experiments, made by Mr. Kirkaldy on rolled bars, gave a maximum breaking strength of 30¾ tons, and a minimum of nearly 20 tons to the square inch. These influences of the manufacture merely on the quality of wrought iron are almost independent of the chemical constitution of any individual bar. For instance, until it be proved to the contrary, there are many reasons for the general belief that the cold shortness of wrought iron is due to the presence of silicon and carbon, and its hot shortness to that of sulphur. A fractional percentage of copper also makes wrought iron hot-short. In truth, there are probably no two bars or parts of a bar, of an exactly similar chemical composition, or in an exactly similar state of molecular aggregation, and, therefore, of an exactly similar breaking strength or elastic limit. Even these are only a few of the elements of uncertainty in structural materials. But, when we further take into account the varied strains of extension, compression, distortion, twisting, and bending, to which mechanical structures are more or less subject; that the work done by a gradually applied load is doubled if this load be applied suddenly; that the impulsive strain of a moving load is generally more or less intensified by vibration; and that the varied shapes and arrangements intended to receive these strains must be often as much fixed by financial as scientific considerations, then the reason that the best engineering practice makes the ultimate strength of a wrought iron structure from four to six times the working load must be even popularly evident.

But these factors of safety are not sufficient. The structure must be tested as searchingly, and as far as is consistent with safety,—as far as is possible without injuring the material and its relation to the structure. In our case this limit is, in the main, given by the limit of elasticity of wrought iron under extension, as this limit is less for wrought iron than that of compression. It is also self-evident, that the mode of testing adopted ought to approximate as nearly as practicable with the kind of stress the object is intended to undergo in practice.

After loading a railway bridge with the greatest passive, or perhaps impulsive load that would ever come upon it in practise, the deflection and the permanent set, if any, are both carefully noted. As a boiler is subjected in practice to a complex train of mechanical and chemical forces that are always striving to break their bonds, its ultimate strength is made from six to eight times the working stress, and it ought to be periodically tested to half its working pressure. Its extension under this pressure is sometimes—and should always be—measured by the volume of water that is pumped in by pressure after the boiler has been filled; while the permanent set is determined by the

difference between the volume pressed out by the contraction of the boiler when the pressure is withdrawn, and the volume of the water that remains in the boiler after the test—allowance being of course made for any slight leakages and sweating at the joints. In first class locomotive works, the deflexion and permanent set of the steel springs are always tested in an apparatus for the purpose. It may here be remarked that, although the designs of all the successful wrought iron structures ever built have been based on the assumption of a limit of elasticity, nevertheless the relation of the permanent set of wrought iron to its ultimate resistance under a given load, is still a subject of some discussion. We have, on the one hand, the testimony of Professor Eaton Hodgkinson, who says, that "the maxim of loading bodies within the elastic limit has no foundation in nature;" and, on the other hand, some appear to believe that iron is even improved by breaking it under at least a tensile stress. Mr. Hodgkinson found that a rod, 10 feet long, and of 1 square inch section, took a permanent set of 0·0005 of an inch under a static load of $1\frac{3}{4}$ tons. Mr. Edwin Clark obtained very similar results. Such a microscopic set, however, could be referred to the elongation caused by the heat generated by the internal friction of the particles, or to the probable fact that these bars were also new; and it is conceivable that they might have taken a slight permanent set, just as new ropes take a permanent set, without injury, when the strain is first applied. There are, indeed, very few forms of wrought iron in which its internal particles are not, *ab initio*, subject to some mutual strain. At any rate, these elongations were very slight, and increased uniformly up to tensions varying from about 10 to 15 or 16 tons on the square inch. Beyond these strains the bars elongated in an irregular manner, until they at last broke. At the same time, as Dr. Rankine remarks, the demonstration by Mr. E. Hodgkinson, that a set is produced by a strain much less than what would injure the specimen, renders the determination of the proof-strength a matter of some obscurity; but Dr. Rankine points out that the best test now known is the not producing an increasing set by the repeated application of a load. Some years ago, Mr. Loyd of Woolwich, made certain experiments which have been cited as proving that a breaking strain does not injure iron, even when this strain is four times repeated; or rather, that after breaking a bar into, say, two pieces, the two pieces are thereby made stronger. In, for instance, experiment 2, the $1\frac{1}{8}$ bar marked C was found to break with $33\frac{3}{4}$ tons, with a stretch of $9\frac{1}{4}$ inches in 54 inches; a piece of this bar then broke at $35\frac{1}{4}$ tons, with a stretch of only a quarter of an inch in 36 inches; another piece of the bar, 24 inches long, was broken at 37 tons, with a stretch of one inch; and at the fourth and last breakage was found to give way at $38\frac{3}{4}$ tons, but without any stretch at all . . . .

There is probably no metal the strength of which is influenced in such a remarkable way by temperature as iron. As M. Baudrimont showed in 1850, the tenacity of iron is less at 100° C. than at 0° C., but at 200° it is greater than at 0°, and these results have been exactly confirmed by Dr. Fairbairn in some experiments on boiler plates, com-

municated in a paper to the British Association. At yet higher temperatures this tenacity is of course diminished; and Seguin has shown that iron, the tensile strength of which could be represented by 100° at 10° C., had this tenacity lowered to 90·5 at 370° C., and to 58·7 at 500° C. In the royal dock-yards of Woolwich and Portsmouth the atmospheric temperature during the testing of each anchor or chain is carefully noted, although the proving houses themselves are kept at a mean temperature of 56° Fahr., by means of stoves, which also thus save the water pipes from freezing. This temperature of course falls a little during the winter and rises in summer, as the heat in the shade generally varies in England from about 76° to perhaps 34° Fahr. The action of frost on iron has not been completely investigated; and Dr. Percy recommends that some accurate experiments on the question be undertaken by the Institution of Civil Engineers. The daily observation of practical men has, however, as in so many other cases, preceded the deeper investigations of science. All workmen know that their tools, such as picks and chipping hammers, which have to undergo percussion in frosty weather, are then more liable to get broken. All chains are well-known to be more subject to snap under the same circumstances. There is always a notorious increase of accidents through breakages, both in the permanent way and rolling stock of railways during frosty weather. It is stated that during the severe winter of 1860-61, 498 rails were broken on the Chemin de Fer de l'Est, from the 11th December to the 31st January inclusive. No less than 258 were broken from the 21st to the 25th of January, during which period the thermometer descended to—7·8°, and even to — 16° Centigrade. General Morin relates that during the northern campaigns of the first empire, artillery veterans used to believe that wrought iron was subject to freezing, and after long winter bivouacs they never began their day's march without striking the gun-carriage axles in the direction of their length and the vibration thus produced was said to "thaw" the iron. An intense cold is also said to have enabled the French garrison of Hamburg to disable the cast iron siege guns by knocking off the trunnions before evacuating the place. Mr. Lenox stated, in evidence before the 1860 Committee, his belief that a cable would stand a test in warm weather that it might not in cold. The crews of the fishing vessels on the coast of Nova Scotia find that the cold renders their cables so brittle that a length of hempen cable is used for the portion out of the water while the anchor end is kept from the vicissitudes of the atmosphere by the usual average temperature of the sea. A few experiments made by Mr. Kirkaldy showed that the breaking strength of a bar is slightly reduced by freezing when a gradual breaking load was applied, but that this difference between a frozen and an unfrozen bolt is much more increased by a suddenly applied load, being 3 per cent. less when frozen. The usual way adopted by French engineers to test rails is, as we have seen, to prove a percentage of the lot by means of a falling weight. Some tests were carried out a few years ago by M. Couche, on a number of rails, of very good quality from the Anzin works. The monkey weighed 300 kilogrammes, and the distance between the supports was

1m. 10. When the thermometer varied from—4° C. down to—6° C., the weight had only to be raised, in an average of twelve experiments, to a height of 5 feet 6 inches, in order to break the rail; but when the thermometer rose from+3° to+8° C., then the weight had to be lifted for a fall of 7 feet 9 inches. Similar experiments conducted in 1860, showed that a difference of temperature from — 4° to + 5° Centigrade, was sufficient to greatly influence the height of fall necessary to break the rail. It is not unnatural to suppose that the particles of iron, after being worked at a heat and allowed to cool and set at a medium temperature, should, when that temperature is lowered, get into a state of mutual strain; or that any initial mutual strain should be thus intensified. The toys made of suddenly cooled glass, known as Prince Rupert's drops, are exaggerated instances of a similar action. The outside portions of a bar, of whatever size, would evidently cool, and consequently contract, first of all. The inside portions would also at last cool; but, having kept the outside portions distended, when the inside does cool, it then becomes a question to be determined by various circumstances, whether it would pull the outside shell into a state of compression, or whether the outside shell will draw the inside into fissures by tension. A somewhat similar explanation is given by Mr. Mallet of the rents caused in the interior of very massive forgings: and this state is probably always induced by the conditions of cooling in a small bar, but with, of course, a smaller range both as to size and temperature. In any case, it is apparent that a ductile, elastic material ought to be less affected by these doubtlessly complicated conditions of tensile and compressive strains. It is, therefore, probable that a hard, harsh, iron would be more affected by frost, than a soft ductile iron; and also, that the breaking strength of both qualities would be less affected by cold than their extensibility.

On Elastic Railway Wheels. By VAUGHAN PENDRED, Esq. C.E.

From the Lond. Mechanics' Magazine, May, 1864.

[Read before the Society of Engineers, May 2, 1864.]

The author commenced by stating that the subject of the paper he was about to read, "Elastic Railway Wheels," or, more strictly, certain expedients for imparting elasticity to these important members of the railway system, was one possessing much interest; indeed, he thought he was justified in saying that the adoption of certain mechanical expedients for seating tyres elastically on the wheels to which they belonged, afforded fair promise of enabling important changes to be introduced into the present system of constructing permanent way: while experience had shown already that by their aid, the duration of engine tyres might be nearly, if not quite doubled, especially on lines of rapid curvature, and that slipping or want of adhesion might be reduced to a minimum.

The paramount object had in view in the formation of a railway, was the reduction to a minimum of those forces which retard the motion of wheel carriages. Setting aside for the moment every other con-

sideration, he might, therefore, define a perfect railway as consisting of two unyielding bars—parallel—absolutely hard on their upper surface at least, and so secured to the sub-structure on which they were ultimately supported, as to be incapable of moving or deflecting under the action of any practical load which they might be called on to sustain. Such a railway, the author stated, would not only reduce the amount of tractive force required for the propulsion of trains or carriages to the lowest limit, but would also possess elements of permanence, which are wanting in any track constructed with timber sleepers, cast iron chairs, &c., under existing arrangements; while, on the other hand, it would be open to certain objections, which have hitherto precluded its adoption.

After describing a theoretical railway track formed of girders laid down on stone blocks, the author went on to say that, were it but possible to introduce the use of stone sleepers, and to permit the ballast to settle its own differences with the sleepers after its own fashion, the expense of maintenance would be greatly reduced. Good ballast always had a tendency to consolidate and become hard, but, under existing circumstances, this quality which ought to be of all things the most desirable, is, on the contrary regarded as a great evil, and no sooner does the ballast become hard, and the timber manifest a tendency really to sleep, than the navvy stirs them both up with his pick. This we are told, is done to secure elasticity; but the ballast was not properly treated if compelled to become the elastic member of the system—to take a part for which it had no vocation, and could not properly assume. If it were not necessary that elasticity should be provided somewhere, a very excellent track indeed, might be made with stone blocks and rails bolted down directly to them, without chairs, and such a track would be infinitely more durable than any other involving the necessity for the use of wood in its construction, always provided that it were exempt from these destructive influences, to obviate the effects of which the elastic element is introduced; in other words, the author believed that the elastic element in our permanent way, as it is called, is the principal cause of its want of permanence, because wood rots and wears out, because nothing has been found as yet which is better than wood, under the given conditions, and because the mechanical arrangements are all so imperfect, and the bearing surfaces so small, that if that motion takes place among the parts which constitute an ordinary railway track which the element of elasticity is presupposed to permit, they quickly become loose, and the true rigidity on which the integrity of the entire structure depended, can only be approximately maintained by an excessive outlay of money and labor.

The author then explained at some length the nature of the action which a rapidly revolving wheel exerted on the rail beneath it, showing that elasticity prevented percussive action, and that the elastic element may be applied with more mechanical propriety beneath the tyre, than beneath the rail, and with equally good effects; and went on to say, that the percussive action of railway wheels traveling at

speed is due directly to their revolution, and is almost independent of any true jumping whatever, and that elasticity operates more as a *preventive* of percussion than as a cure. In other words, the suddenness of strain characteristic of percussion was prevented, as he had just endeavored to show, instead of being absorbed or taken up, as might be the case, if the wheels were supposed to be lifted off the rail for a space and suffered to drop.

No means had yet been discovered for preventing the effects of percussion taking place between revolving wheels and the rail which supported them, but the introduction of elasticity, somewhere, into the system; and experience showed that this elasticity could be introduced with the utmost advantage into the wheel, or beneath the tyre, instead of, or in addition to, elasticity beneath the rail or the sleeper, and if no better expedients for securing this necessary element in track could be devised than those already in use, which depended for success on soft ballast, or costly destructible timber, then he believed it might yet be found advantageous to seat rails as rigidly as possible on heavy stone sleepers, and to adopt some expedient beneath the tyres of not the engine alone, but every wheel of the train, which should supply that elasticity which would, in such a case, be absent from the permanent way. This was, however, a point open to discussion, and he would therefore proceed to the consideration of the various expedients which had been proposed from time to time for seating tyres elastically, or in some one way imparting resilience in no ordinary degree to the wheels of vehicles. The idea of an elastic wheel was by no means new. Patent Office records showed that, with the first notion of propelling carriages by the adhesion of the wheels on which they rested, men sought to increase that adhesion by enlarging the surface in contact with the ground, either by using a very broad wheel, or by adopting certain expedients which would permit the wheel to depart slightly from a true circular shape, and become more or less oval.

The author then gave a short history of the elastic wheel theory, showing that Mr. T. Neville, the inventor of the multitubular boiler, had been one of the first to propose to use elastic tyres to obtain increased adhesion, as early as 1825. After describing many inventions, the author went on to say that all those expedients were indirect, and more or less unlike the arrangments adopted in modern railway practice; but he found that, in 1837, Sir George Cayley took out a patent which included nearly all that had been done since, in the idea at least, if not in the actual mode of application. Not only this, but he found that, in 1831, he had suggested, in the pages of the *Mechanics' Magazine*, an improvement in railway wheels, designed to reduce their wear, which possessed many points in common with the Griggs' wheel. Sir George writes, "If the wear and tear of railway conveyance be found too expensive, owing to the friction caused by such high pressure and great velocity, and that the use of springs to these carriages are not sufficient to remedy the evil, I think it probable that a dovetailed groove, filled with hard oak, driven in small pieces endways within the rim of the wheels, and then turned off in the lathe, might be serviceable and

could be cheaply renewed; these pieces might be secured by a fox-wedge as commonly practised in similar cases." In Sir George Cayley's patent he describes a wheel "made with a deep flange, and to the opposite side of the tyre is secured a ring or annular plate of less depth than this flange; the space between the flange and the ring is to be occupied by a filling up of hoof or horn, or tough woods, or of other partially elastic substances, suitable for giving a slight degree of elasticity to the periphery, for diminishing the effects of percussion.

In 1839 Mr. W. Bridges Adams brought out a very ingenious elastic wheel, with spokes composed of steel rings, while the tyres were seated on wood blocks or felloes. Mr. Adams' wheels were tried under his own supervision, and at one time he had several sets running. The results obtained were not quite so satisfactory as he desired, principally because of certain practical difficulties met with in the construction of the wheel.

Setting aside one or two isolated instances of the use of elastic wheels, he found that Mr. George S. Griggs, locomotive superintendent of the Boston and Providence Railway, United States, was the first to seat the tyres of his driving wheels elastically on a practical scale. The means he employed to effect this purpose are very simple. American locomotives almost invariably have cast iron driving wheels, fitted with wrought iron or steel tyres. The Griggs' wheel was made with a number of transverse dovetailed grooves, cast in the periphery; into these grooves blocks of thoroughly dried, hard wood, such as fine-grained old oak or hickory, were driven, so that the grain of the timber ran parallel with the axle, and across the periphery of the wheel. When these blocks had been driven into their places, the wheel was put into the lathe, and so much turned off them that they only stood up above the surface, an eighth of an inch or so. The tyre, previously turned up true inside and out, was then heated and placed on the wheel, care being taken not to scorch the blocks on which it then bears, no metallic connexion of any kind existing between the wheel and its tyre.

The Griggs' wheel was brought out in 1857, and has been, and is extensively used in America, with the best results.

The "Canton," an engine on the Boston railway—as an example from many cited by the author—had run 133,373 miles; the tyres were now reduced to $1\frac{1}{4}$ inch in thickness. The four coupled drivers are 5 feet in diameter, and loaded with nearly 14 tons.

The thinness to which tyres seated elastically on Mr. Griggs' plan had been worn was very remarkable, and such as we dare not attempt on a rigid seating. Yet the fracture of an engine tyre on the Boston and Providence road is all but unknown.

Mr. Bridges Adams had lately introduced a very elegant arrangement. The rim of the wheel is turned slightly convex; the tyre is rolled with a groove on the inside. Into this groove two hoop springs, made in segments, and each about one-third of an inch, thick are placed, so as to break joint. These hoops are made of steel of the best quality. They bear only at their ends on the sides of the groove in the tyre,

and the convex wheel rim, resting only on the centre of their breadth, is supported elastically. The tyre is secured in its place by a ring of iron sprung in at the back of the wheel. In a more recent modification the rim of the wheel is turned perfectly flat, and only a single spring hoop is used, thickened in the centre of its cross-section. This hoop is made in one piece, with the ends abutting, and forced on the wheel after being placed in the tyre by water pressure. The tyre of the first Adams' wheel, when unloaded, can be rotated on the wheel by hand, and a slight rocking motion of the wheel within the tyre is permitted, which enables the tyre always so adapt itself to the rail in the best possible manner. Wheels so fitted seldom or never slip. The area of surface between the spring and the tyre, and the spring and the wheel rim, is so great in consequence of the circular shape of the parts, that the wheel cannot turn with ease within the tyre when loaded; and this last, being elastically supported, yields slightly, becoming a little oblate, instead of remaining truly circular, and thereby acquires a better hold of the rail, consequent on an increase of bearing surface. Tyres usually break from tension; but the tyres in this case, being elastically supported, can scarcely be said to be in tension at all, and it was, therefore, very improbable that they should break even in intense frost. Indeed, two of these tyres had been cut across for the purpose of experiment, and in that state actually did three days work drawing coal trains.

The first trial of Mr. Adams' spring wheel were made on the North London Railway, in 1858 or 1859. The spring tyres were applied to a set of disk wheels. The tyres were of Staffordshire iron, and ran a distance of 104,000 miles with very little wear. Low Moor tyres, rigidly seated on the same class of wheel, were completely worn out in running the same distance.

The next trial was on the Eastern Counties line, in the early part of 1859, when Staffordshire tyres were fitted, on the Adams' system, to the four coupled wheels, 5 ft. 6 ins. diameter, of a goods engine. The tyres were of the worst possible quality, and the springs very little better. These last broke and set so that they had to be removed, and thus far the experiment was a failure. In September 1859 however, a pair of Cooper & Co.'s tyres, elastically seated on the same principle, were applied to the leading wheels of a tank engine, working the Woolwich branch of the same line. This branch abounds in sharp curves, one of them being but 5½ chains radius, and another but 4¾ at the sharpest part. The engine had a 12 foot wheel base, and the load on the spring wheel was 7½ tons.

The engine began working on September 21st, 1859, and the wheels ran up to July 26th, in the following year, a distance of 25,240 miles before being turned up. The wheels were put under again in September, 1860, and ran up to January, 1861, a further distance of 8776 miles. At this time the tyres got quite loose from the breakage of one or two of the springs, and the engine requiring repairs generally, was taken out of work for a time, and the spring tyres were not replaced. The same class of tyres put on in the ordinary way require re-turning

when they have run but 8000 to 10,000 miles, when the flanges are found much worn; afterwards they must be turned up every 4000 or 5000 miles until they are worn out.

Mr. Adams' tyres were largely used on the St. Helen's Railway, with great advantage. The author detailed at length the results obtained in the daily working of these tyres, showing that when Staffordshire iron was elastically seated, it actually gave a 3-5ths greater mileage than Krupps' steel rigidly seated.

The last form of elastic seating to which he would call attention had been recently patented by Mr. Mansell. The wheel rim is first turned up, and then the inside of the tyre is bored out, so that when put in place a space of about one inch intervenes between the two surfaces all round. This space is filled with a ring of wood—teak being used by preference—on to which the tyre is forced, and secured in its place by six segments of iron on each side of the wheel. A groove is turned in each edge of the tyre, and a similar groove in each edge of the wheel rim, into which raised ribs on these segments enter, while pins pass through the wood and secure the whole in place. These wheels have been so short a time in use, that many data do not exist about their performances yet. A good many of them were running on the South Eastern Railway. On the Great Eastern Railway one engine had the leading wheels fitted on this plan; they had been running some months, and showed little signs of wear.

An interesting discussion followed, in which Messrs. W. B. Adams, Z. Colburn, Ordish, Musey, and several other gentlemen took part. The paper was fully illustrated by diagrams.

On the Bursting of the Bradfield Reservoir. By Hewitt Davis.

From the London Builder, No. 1108.

I have read with much attention the evidence that has been given as to the cause of this sad accident, and must confess that I think as yet very little light has been thrown on it.

I have for many years been engaged as a draining engineer and government inspector in the drainage of clay land, and have had to observe the wonderful contractile power that clay exhibits in drying. I may instance the cracking of clay land in autumn, and our ability to make the strongest soil porous by inserting 4-feet drains every 27 feet. This experience leads me to think that this accident may have been caused by the drying and cracking of the clay puddle in the embankment, consequent upon the sinking of the water in the reservoir during this singularly dry winter, and its exposure while in this state to the sudden rise in the reservoir after the heavy rains at the time of the accident.

The reservoir was formed by an embankment carried across the valley, 90 feet high, for the purpose of collecting the water that came down in the wet seasons; and the embankment has been made watertight by having a puddle of clay carried up along its centre. This

puddle, it appears, was made 14 feet thick at bottom; tapered off to 4 feet at top; and was supported, back and front, by the material of the embankment, which it is admitted was formed of loose earth; so that the puddle stood like a wall 90 feet high, 14 feet thick at bottom, tapering to 4 feet at top, exposed to wet and draft as the water rose and fell in the reservior. This puddle was made of clay worked wet, and would be dependent for its continuing imporous upon its retaining the water incorporated in it. Now, the question with me is, has this clay wall remained in a condition this winter to retain its moisture, and not undergone the change that takes place whenever clay parts with its moisture? I think not. The amount of rain-fall in the Midland Counties during the 6 preceding months to the accident had been remarkably small,—only 6 inches, or less than half the usual quantity,—and the springs throughout the county had been remarkably low. The ponds usually overflowing at Christmas were not half filled in February; and wells were dry that were never before known to fail at that season: so that the contents of the reservoir must have been reduced very low, and the embankment have been exposed to an unusual exhaustion of its moisture; and while in this condition the heavy rains came, and the water suddenly rose in the reservoir, filling the fissures under great pressure. The effect of this, I take it, has been that the water has been forced through the puddle. Let me ask, is this not likely to have been the case, and the cause of the accident?

MECHANICS, PHYSICS, AND CHEMISTRY.

On Ailanthine and the Ailanthus Silk-Moth.

From the Journ. Soc. of Arts, No. 572.

The following paper, by Dr. Robert Paterson (corresponding member), was read before the Botanical Society of Canada:—

There are few individuals who have not watched the interesting changes which takes place in the larvæ of the *Bombyx Mori*, or common silk-worm, from the point of its exit from the egg until it has reached its full butter-fly existence; and many there are who have been sadly disappointed at the mortality which comes over a brood of silk-worms in a single night from some cause or causes unknown, and consequently unremediable. Such epidemics are continually occurring in China as well as Europe, and constitute one of the greatest obstacles to the introduction of the culture of the silk-worm into England. What occasions this sudden decimation of these insects has never been determined, but has long led to a wish, on the part of those interested, that a more hardy breed of silk producing worms could be introduced into Europe, even though the produce was coarser, and of a worse color than the ordinary mulberry silk.

Recent information, through our missionaries in China, leads us to the knowledge that there is a considerable number of worms used by the Chinese, in different districts, for the production of various qualities

and coarseness. These varieties of silk are used in China principally for the manufacture of dresses for the peasantry. Of late, however, some of these have reached this country, and have been considered durable and excellent. Could we but rear such silk in our country, as we hope shortly to be able to show that we can do, how much of the present overwhelming distress, which is visiting our manufacturing districts in consequence of the American war, might be avoided? Such material if not used along, might be mixed with cotton or wool, and thus many new and beautiful, if not durable, fabrics might be produced.

In 1814 Dr. Roxburgh* published an interesting memoir on the silk-producing moth of the East Indies, and soon afterwards the Arrindy or Palma Christi silk-worm was introduced into Europe. The castor oil plant, in this climate and in the north of France, is but a delicate shrub: in the south of Europe, however, where the temperature never reaches the freezing point, it becomes a tree of very striking aspect, with large and rich tinted foliage. In such districts, therefore, the Arrindy moth thrives well, having plenty of food, undergoing its changes rapidly, and yielding five or six crops annually of silk of excellent quality. What was required for our climate, however, was an insect which, while sufficiently hardy to stand our cold springs and autumns, would also be regardless of storms, rain, dew, &c. Such a worm was first sent to Europe by the Abbé Fantoni, a Piedmontese missionary in the province of Shan Tung. He sent some cocoons, immediately after the first gathering in 1856, to some friends in Turin. The name of the tree on the leaves of which they lived was to him a mystery, but he described it as being like the leaf of an Acacia: so when the young brood hatched, various and many were the plants tried for their food, until the leaves of the *Ailanthus glandulosa* were presented to them: these they immediately ate greedily, and always preferred them afterwards to any other kind of food.

There can now be little doubt but that the Arrindy or Palma Christi moth introduced into Europe from Dinajepore and Rungpore in Bengal, in 1854, and the Ailanthus moth introduced into Europe from the province of Shan Tung in China, in 1858, are one and the same animal. The insects introduced in 1854 were delicate, and did not stand much lowering of the temperature; besides, the tree on which they fed perishes at 32° or 33° Fah. The insects introduced in 1858 were hardy, stood rain and cold, and the tree which they preferred is a hardy one in our climate. Those introduced in 1858, from China, would not eat the Palma Christi, and very naturally it was believed that they were different insects; upon examination, however, they turned out to be the same. Their changes, the color of their larva, the character of the cocoon, the kind of silk, and the characterizing marks of the moth itself pronounce them at once to be the same animal. But how have these animals acquired such different habits of taste? This can only be explained on the supposition that a long period of hardening in a temperate climate, like the province of Shan Tung, would produce in course of time a more hardy progeny, feeding habitually on a common

* Linnæan Transactions, Vol. vii.

plant of the country, while the more effeminate brood of Central India preferred as food the leaves of a plant which will only flourish in warm latitudes. Unless specific distinctions exist, it is clearly a bad plan to distinguish an insect from the peculiar plant it eats, for this may be a simple point of preference—if it cannot get the one it will eat the other, and thrive on it; besides, a long period of hardening will often enable an animal to live and thrive on a vegetable very different from its native food. We need only instance the ordinary *Bombyx Mori*, or common silk-worm, the finest varieties of which, after passing a year or two in our climate, will live and thrive, and spin beautiful silk on the common lettuce. Of the tree on which the ailanthus worm feeds, it may be necessary here to speak shortly; we shall have to describe the animal itself more fully afterwards.

It appears that the tree was originally introduced into this country by the Abbé d'Incarville, in 1751, as the "Vernis de Japon" tree, or that which yielded the famous Japan or China varnish. This turned out, however, to be a mistake, as the true Japan varnish tree has since been introduced into Europe. Since this latter introduction, the *Ailanthus glandulosa* has been known as the false varnish tree. It is a hardy plant in our climate, standing severe winters well, and producing an abundant crop of leaves, especially from young shoots, in early summer. It has no especial partiality for particular varieties of soil, thriving as well, and producing as abundant a crop of leaves, in the most barren soil as in the richest loam. It seems equally indifferent, too, as to the characteristics of the atmosphere in which it lives, healthy young trees being observable in the squares and smoky environs of London. The advantages of a plant such as this in the rearing of a hardy animal on its foliage need not be pointed out. Throughout France, generally, this tree flowers and seeds freely, and the seed sprouts and grows readily in Great Britain; but in addition to this method of propagation, another exists in the roots, which if cut into pieces like the potato, spring forth and grow luxuriantly; no plant, indeed, can be more easily raised, or more easily increased when grown than the *Ailanthus glandulosa*. But to enable this plant when grown to yield a proper supply of food for the ailanthus worm, it is necessary to cut it down and grow it ozier-like. In this way young shoots spring forth abundantly, and bear large and delicate leaves fitted for the young worm, and greedily devoured by the older ones. They have an additional advantage also that when the insects are placed upon them in the open air they are more easily protected by nets, &c., from the depredations of birds, insects, &c.

So much for the plant on which the animal feeds. Let us now turn to the insect itself:—I have already stated that the ailanthus silk-worm was introduced into Europe in 1856. Its cultivators have not been idle since that time, as we find that M. Guérin Méneville endeavored to introduce this worm into France. His first experiment did not succeed, but the following year he reared a satisfactory crop of cocoons in the open air; this, however, and all the efforts of the Société d'Acclimatation of Paris were not sufficient to effect the general introduction

of the animal into France. It became necessary for him to show that agriculturists might derive a profit, and a good one, from the rearing of this insect.

Energetic, and thoroughly convinced of the success of such an experiment on a large scale, he induced personal friends to experiment on a larger scale at Toulon, in Provence, and at Chinon (Indre-et-Loire), the one being nearly in the south, the other in the centre of France.

At Chinon, for instance, 4500 worms were placed upon flourishing thickets of ailanthus, which had been cut down and grown as bushes, with that intention. Their development progressed satisfactorily, and they yielded 3515 excellent cocoons, after suffering, without injury, rains, heavy storms, and the attacks of birds and insects. The result of the experiment was a loss of about a fourth part, while the average loss of mulberry silk-worms is about one-half.

M. Méneville, after some careful experiments and calculations, which were submitted to the imperial government, has thus stated his profit and loss account, on the rearing of ailanthine, or the silk of this worm, produced in districts south of Paris.

	Francs.
Twelve acres of ailanthus copse, share of expense of planting and annual expense of keeping up, ...	2030
Average of receipts from two crops of ailanthine, . .	9945

which leaves a balance of 7915 francs for the twelve acres, or in round English numbers, £330 for twelve acres, or £27 10s. per acre. In India and China there are said to be six crops of silk annually; in the south of France two or three crops, but in the north of France and Great Britain two at most, and more securely one crop might be relied on. Let us take one good crop, and see how our profit and loss account would stand in Great Britain. The half of £27 10s. or £13 15s. would be the result, or about it; and be it remembered, for land, that after the planting of the *ailanthus* it requires no manure or tillage whatever, and the kind of soil being that on which nothing else would grow, provided always that it has as sheltered and sunny an exposure as possible. It always occurred to me that the climate of Canada would be especially favorable for the growth of ailanthine. The insect and the plant on which it feeds will stand any amount of cold; and when the Canadian summer arrives, rapid growth would take place in the tree, followed by hatching of the worm; in this way food would be speedily produced for the young brood, and two, if not three, crops of silk taken from the trees during the season. The experiment is one worthy of trial.

In England and Scotland, for the last two years, some experimenters have been at work, but as yet without any quantitative result. In the spring of 1862 I received, through the kindness of a friend, fifty eggs of the *Bombyx Cynthia;* they hatched in about ten days after their arrival; they were fed with cut branches of *ailanthus;* kept in the ordinary temperature of the atmosphere, but under glass. From the fifty worms (for the eggs all hatched) with all my inexperience, I had thirty-five large and fine cocoons, being a result not far short of that

in the central districts of France. With more experience, and with growing plants prepared for the trial, I do not fear for the result of a quantitative trial in Scotland at any future year.

It is my intention, in describing this insect, to follow the different changes which it undergoes from the egg onwards until we arrive at the characteristic moth itself, from which distinctive marks and peculiarities are chiefly taken.

The Eggs.—These are about the size of a large pin head, twice as large as those of the mulberry silk-worm, with which we are all familiar. They are yellow-colored, equally large at both ends, flattened from above downwards, and with a depression in their centre. They soon change their color to a greenish-black, the color becoming more marked the nearer the point of hatching is at hand. The caterpillars are hatched from ten to fifteen days after the eggs are laid, according to temperature.

The Caterpillar.—When the worm first escapes from the egg it is exceedingly minute; the color of the segments of its body at this early stage is obviously yellow, but there are so great a number of black spots and dark colored tubercles over it, as to give the impression that it is of a black color; during the second period, that is to say after the first change of skin, the yellow color becomes more marked, but the spots and tubercles are still black. During the third period they become nearly pure white, arising from the presence of a white mealy secretion over their bodies, destined, obviously, to protect them from rain or dew, as water will not fix on it; the spots and points of the tubercles are still black or bluish black.

During the fourth period the body is at first white, but gradually changes to a pale green, the tubercles assuming the same color, and soon the head, the feet and the last segment become of a golden yellow; the flowery secretion still, to a certain extent exists and there are always black points upon the segments or rings of the body.

During the fifth period the emerald green coloring becomes more intense, the points, as to segments, assume a soft black color, and the extremities of the tubercles a marine blue. The caterpillar grows rapidly during this stage, eats largely and greedily till it attains the length of from $2\frac{3}{4}$ to 3 inches long; it then ceases to eat, becomes torpid for a few days, and after fastening a few leaves together at the extremity of a leaf or branch, it begins its cocoon. Such is the general character of the changes which this caterpillar undergoes; but to enable those who may follow out this inquiry to know when these changes may be expected and the size of the animals in them, I will give a short table of my own experience, and that of my friend Dr. Gudwad, both in Scotland:—

Eggs hatched, 28 to 30th June,	size	¼ of an inch.
First change, 7 to 9th July,	"	½ "
Second change, 13 to 15th July,	"	1 "
Third change, 20 to 22d July,	"	1¼ "
Fourth change, 28 to 30th July,	"	1½ to 2 inches.

From this time till the period when it begins to spin it rapidly grows

till it reaches from two-and-a-half to three inches long, depending upon the abundance and quality of its food.

The experience of my friend Dr. Gudwad is as follows:—Eggs hatched 19th September; 28th September first change began; 5th October second change began; 12th October third change began; 21st October fourth change began; 3d November began cocoon. The temperature ranged from 47° to 55°.

The Cocoon.—I have already remarked that after a short period of torpidity when no more food is taken, and during which the remains of the undigested food are passed by the worm in abundance, it begins its cocoon by fastening some threads of silk to the end of the branch or leaf stalk, and after binding some leaflets together, it spins its cocoon in the hollow thus formed. The color of the silk is of a yellowish-brown very like, indeed, to that of a decayed leaf. In weaving its cocoon the worm leaves at its lower extremity an elastic opening for the exit of the moth. The threads at this opening are not cut across, but simply turned and laid one over another. The silk of this worm has not as yet been unwound in a continuous thread; this, doubtless, arises from the substance which glues the threads together, requiring some other solvent than the warm water which so readily effects the solution of the gummy secretion of the mulberry silk. This however, cannot long remain undiscovered in this country, as a chemical solvent for this secretion will doubtless ere long be found.* In China even there is reason to believe that this has been accomplished, as the last examples of *ailanthine* from that country are stated to leave no doubt of their having been unwound from the cocoon. Even the carded silk of this worm is abundantly used. In China it forms the most durable dresses of the peasantry, dresses which are often handed down from father to son. In France this "flosille" or floss is abundantly used for weaving with thread and wool and in the manufacture of fancy stuffs. At Roubaix, Nismes, and Lyons, it is imported from abroad in large quantities to the extent of 1,290,000 kilogrammes annually.

Mons. Geoffrey St. Hilaire, President of the Société d'Acclimatation of Paris says:—"Here is the report of the weavers at Alsace, who have made use of ailanthus silk. M. Schlumberger has found the cocoon very easy to card and spin; the thread obtained is less brilliant, strong and rough; it left no residue, not more than in combing the thread. It is most excellent stuff for use in all manufacture where *burre* is employed. The cocoons are easily cleaned, and they will take a good dye. This culture made on a great scale, will furnish in abundance a finer and stronger floss than the mulberry silk-worm. The worm remains in the cocoon in the chrysalis condition for from twenty-six to thirty days, at which time the moth makes its appearance, coming quickly and easily through the valvular opening at the extremity of the cocoon. At this time its wings are moist, soft, and folded up;

*It has been stated by some that the cause of the silk not winding off results from the slanting opening at the bottom of the cocoon, admitting water, and thus sinking it and breaking the thread. This explanation is not satisfactory and is inconsistent with fact.

and, naturally, upon emerging from the cocoon, it seizes hold of the lower part of it, thus allowing its large wings to drop, become unfolded and stiffen. If this precaution is not taken when the moths are allowed to exit artificially, their wings never expand, but remain crumpled up, the moth never regaining much activity with its wings in this state, and seldom connecting itself with the opposite sex. In rearing these moths, therefore, it is of consequence to observe that upon their exit from the cocoon they have some substance on which they can climb up and allow their wings to hang down and become expanded.

The moth has been long familiar to us, in collections of Chinese butter-flies, brought to this country. It is large, the expansion of its wings being about five inches; the head and antennæ are greyish-brown, the latter strongly pectinated; thorax and abdomen lighter grey; wings with a broad transverse light-colored band near the middle, the space within which (forming nearly an equilateral triangle) is brownish-grey, and that without ash-color, running into brownish-grey at the margins of the wings. Just within the margins there are two narrow brown streaks running parallel with them, somewhat interrupted before reaching a black spot near the apex of the superior wings; this spot is surmounted by a white crescent, and a zigzag white line runs from it to the tip. The basel portion of the superior wings is traversed by an ash-colored bar commencing on the posterior edges next the shoulder, and after continuing in nearly a straight line for about half-an-inch is suddenly deflected and terminates on the anterior margin; between this bar and the transverse scapula line there is a pale longitudinal spot surrounded with black. The under wings likewise bear a similar spot but more crescent-shaped, and towards their base there is an ash-colored arched bar bounded on the outer side with black. The under side differs principally in being paler and destitute of the angular and arched bars at the base of the upper and lower wings.* These moths when in health and especially in sunshine, connect themselves and lay eggs in a few days. If they do not develop their wings, or the temperature is low and without sunshine, the males do not seek after the females, hence the eggs laid are often, under these circumstances, unproductive.

* Sir H. Jardine's description of *Saturnia Cynthia*, and corresponding in every particular to *ailanthus* silk-moth.

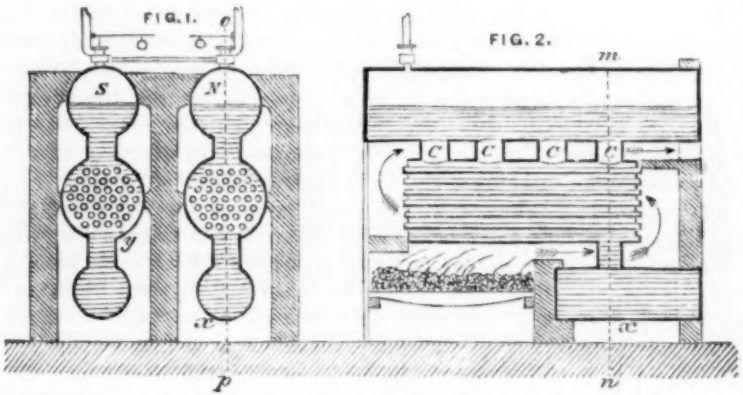
Preservation of Wood.

From the London Chemical News, No. 227.

The following composition is recommended to protect the bottom of posts, pailings, and tubs set in the earth:—Forty parts of chalk are added to fifty parts of resin, and four parts of linseed oil, melted together in an iron pot. One part of native oxide of copper is then added, and one part of sulphuric acid, is cautiously stirred in. The mixture is applied hot with a strong brush, and forms, when dry, a varnish as hard as stone.—*Neues Jahrb. für Pharm.*, bd xx., s. 235.

Steam-Boiler Explosion at Messrs. Cornelius & Baker's Manufactory, Philadelphia—Verdict of the Coroner's Jury.

In accordance with your (Coroner Taylor's) instructions, we, your jurymen, have examined carefully the premises of Messrs. Cornelius & Baker, on Cherry Street, the scene of the late disastrous explosion; have heard the testimony of the witnesses brought before us, and, having exercised our best judgment in the consideration of the case, are now prepared to render our verdict, prefacing it by a statement of the facts as presented to us, and detailing our deductions from all the evidence variously accumulated.



The boilers which exploded were designed* and built by Messrs. Morgan & Orr, of Philadelphia, and had been in use about two and a half years. There were two of these boilers set side by side, but working independently of each other; of these, the one towards the south was built and put in operation a few weeks before the other, which we may call the north boiler. Each of these boilers consisted of three parts, or three cylindrical boilers, placed horizontally one above the other, and called, in the order of their superposition, the mud-drum at the bottom, then the tube-drum, and on top the steam-drum. The mud-drum was back of the grate bars, and was 24 inches in diameter and 6½ feet long. The tube-drum extended over the fire, and was 41 inches in diameter and 10 feet long. The steam-drum was 36 inches in diameter and 14¼ feet long. The water line was at the middle of the steam-drum, which part was therefore half full of water: the tube-drum and mud-drum below this were of course, intended to contain water only.

The mud-drum was connected with the tube-drum by one neck 12 inches in diameter and 12 inches long, while the tube-drum was con-

* By this word, the Jury did not mean to say that the boiler was originally designed or devised by Messrs. Morgan & Orr, (for it was in testimony before them that the original design or arrangement was by Mr. Gavitt,) but that it was selected or designated for Messrs. Cornelius & Baker by Messrs. Morgan & Orr. Ed.

nected with the steam-drum by four necks each 12 inches in diameter and 8 inches long.

The tube-drum contained 58 tubes of wrought iron each 3 inches internal diameter, running from end to end; through these the fire passed, the water surrounding them. There was thus left water spaces between the tubes themselves, and between the outer row of tubes and the shell of the boiler of only 1 inch in each case.

The fire from the furnace was made to pass under the tube-drum and over the mud-drum to the back end of the former, then returned through the tubes of the tube-drum to the front of the boiler, and then passed over the upper half of the tube drum and under the steam-drum to the chimney.

Water was supplied to the boilers by a No. 5 Giffard injector, through pipes passing from the front of the boiler to the back of the mud-drums, which they entered at the lowest part. Water was supplied to each boiler separately, and cocks were arranged to control the filling of each boiler independently. There was no water connexion from boiler to boiler other than these feed-pipes, which in practice were not used to connect the boilers, but each one was fed separately.

The steam was taken from the front ends of the steam-drums by cast iron pipes. To each boiler over the pipe was attached a safety-valve, and the steam passed to the engine through a single pipe, but could be controlled by both cocks, so as to enable each boiler to be used independently of the other. To determine the water level, each boiler was supplied with three try-cocks: one at the working water level—*i.e.* at the centre of the steam-drum—one four inches above and another four inches below this point. There were no other means of observing the water level beside these cocks: and the lowest of them stood nine feet from the floor, and could only be reached by means of a rod or by climbing up to it.

The firm seemed to have placed great reliance upon steam-pressure-gauges, one of which was situated in their office in a conspicuous place, while the other was near the engine: but so far removed from the boilers that it could not be seen by the engine tender when he was at the boilers, the engine and boilers being at least fifty feet apart. Thus the engine tender had no means of judging as to his steam-pressure when working at or near his boilers.

From the evidence presented to us we learn that some three months previous to the explosion, the mud-drum under the south boiler was found to leak in one of its cast iron heads and was consequently removed and sent to Messrs. Morgan & Orr for repairs. But upon examination, they (finding the iron very thin from corrosion) notified Cornelius & Baker of the fact, and the firm ordered a new mud-drum to be put up in place of the old one.

The mud-drums of both boilers were said to have been made from pieces of boilers which had been previously in use for two years, but which were considered to be in good condition, and, in the opinion of the builders, better than new iron. We are surprised that, at the time of the substitution of the new drum, there was no rigid examination into the

condition of the mud-drum under the north boiler, which was also made of old iron, and had been in use nearly as long as the one which had given out.

The mud-drum under the north boiler at the time of the explosion was therefore about the same age, or had been in use about as long as the old mud-drum under the south boiler, and after the explosion was found to be very badly corroded on the inside, the iron in some places being less than one-sixteenth of an inch in thickness; this in places where there seemed to have been no outside rusting of the drum. The outside was also found to be much corroded in parts which had been in contact with the walls.

The engine-tender has been examined, and seems to understand his business, and no testimony has been submitted to us that would in the least degree tend to show any habitual neglect of his duties, but, on the contrary, his character for sobriety and close application to his work seems fully substantiated.

From his testimony, corroborated by that of other witnesses, we learn that on the morning of the explosion he was behindhand with his steam which was not up to its working pressure when the engine was started.

He was not putting any water into the boiler at the time of the explosion, nor for some time before. The only testimony we have of the height of the water at this time is from the engine-tender himself. He states that he had a full supply of water, and that he tried the cocks just before leaving the boiler-room. Other witnesses were in the boiler-room a few minutes previous to the explosion, and had there been any water indicator which could have given a visible indication of the water level, their testimony would have been of the greatest value.

The engine-tender was away from his post at the time of the explosion, and no competent man had been left in his place. The only instructions were to a workman in the stamping-room near-by to watch the steam-gauge, which was in the engine room out of sight of the boilers, as before mentioned, and your jury do not think that the man to whom the direction was given was a competent person to take charge of the boilers.

In the absence of any evidence of gross neglect on the part of the engine tender, and assuming that the water was at its proper height a few minutes before the explosion, and that the steam-pressure was moderate, as was proved by many witnesses, we must direct our attention to the probably weak parts of the boiler, in looking for the cause of its disastrous explosion.

The mud-drum under the north boiler has shown itself to be less than 1-16th of an inch in parts which did not break, and was presumably weaker in parts destroyed by the rupture. No one can maintain for an instant that it was in a fit condition to bear the ordinary working pressure.

We believe that this was the first part to give out, but as the bursting of a mud-drum does not always cause an explosion of the boiler, and as the violent explosion in this case seems to have occurred in the

tube-drum of this north boiler, which was literally torn to pieces, we must infer that, as the water fell, by reason of this primary rupture in the mud-drum, to a dangerous level, the water was driven away from the tubes by excessive ebullition; that thus some of the tubes being bare and unduly heated, the water temporarily driven away returned, and coming upon the heated tubes, caused the explosion.

We believe the explosion of the south boiler to have been the result of injuries inflicted by the bursting of the north boiler.

With regard to the form of the boiler and its liability to explosion, your jury believe, from the weight of the expert evidence brought before them, that this form of boiler is not safe, and that, by excessive firing and even moderately low water, (we mean before the water has fallen so as to uncover any of the tubes,) the water can be driven away from parts of the tubes, laying them bare for a time: and that, upon any slacking of the fire and the return of the water upon the tubes, an undue strain will be brought to bear on the boiler.

We think that the boiler is constructed on erroneous principles, for the following reasons: Granting that the cylindrical form of each part gives strength, the cylinders are materially weakened by the holes made for the connecting necks.

We hold, too, that the mode of applying the heat is entirely wrong. The greatest heat of the fire comes in contact with the under side of the tube-drum at its front end; it then, at a gradually decreasing temperature, passes upward in the system through the tubes, and thus through the very water put in motion by the intense heat at the lower part of the tube-drum, and finally passes away in contact with the steam-drum above. The capacity of water to take up heat from the products of combustion is, as we know, proportional to the difference of temperature in favor of the latter. In other words, water at 80° can take up more heat from products of combustion at 300°, than water at 300° can.

To use the heat, therefore, to the best advantage, the products of combustion should be made so to pass through the boiler system as to bring the gradually cooling gases in contact successively with cooler portions of the water. We express this opinion only to show that we do not think this boiler has any claim for economy to compensate for its complicated form, and we judge that a single cylinder boiler partly filled with tubes, with the water-line in the same cylinder, would be at once safer and more economical than this form of boiler. We are aware that many of these boilers are in use, and we would urge the greatest caution in their management. They may be moderately safe when the boiler capacity is large as compared with the work to be done, but are not safe where the consumption of steam requires heavy firing to keep up the pressure. Living as we do among steam boilers, knowing that in our most crowded thoroughfares they are under the very pavements, it is due to the safety of the community that strict municipal surveillance should be kept over the condition and the structure of the boilers used. We would, therefore, in the strongest terms urge the action of the authorities in reference to the use of steam.

There seems now to be no system of inspection in existence. Any form and any condition of boiler may be put in use, and when an explosion occurs, the fact that so many similar boilers have not exploded shields from blame those who would justly be held accountable for the destruction caused.

Messrs. Cornelius & Baker, are like thousands of others using steam power, not professional engineers; they therefore depend upon the advice of others in regard to all matters connected with their steam generating apparatus. Their responsibility would seem to rest with the choice of advisers and with their close supervision of those under them in responsible positions. We believe that they have in the latter case been careful in selecting an engine tender, and watchful over his actions; we can find no testimony to impeach the sobriety or competency of this engine tender; but we believe he has not made as careful and as frequent examination of the internal condition of the mud-drum as he should have done; but in this he is not singular; we have heard, and are hearing daily since the explosion, of mud-drums giving out in various parts of the city, and the warning has led to an examination of others which, although they have not exploded, are too thin to be safe.

All this points to the necessity of some reform in the laws regulating the use of steam; competent inspectors, free from all political influence, should be in charge of the matter, with power to call on the best engineering talent of the country for advice: and laws should be passed regulating the number and character of danger indicators which should be attached to each boiler as is customary in France and other countries.

For the safety of our citizens it has been found necessary to provide building inspectors, and to pass laws governing the thickness of walls and the arrangement of buildings, but no law is brought to bear upon the regulation of steam power.

The sooner this is done the better for the community at large.

We therefore render the following verdict. That on the 25th of April, 1864, William Bartholomew, Alfred Schäffer, Samuel Davis, Thomas H. Albertson, George Hess, J. L. Snyder, John Porter, and Anthony S. Fry, came to their death by reason of injuries inflicted by the explosion of a steam-boiler in the establishment of Messrs. Cornelius & Baker, Cherry Street, below Ninth, Philadelphia, the primary cause of said explosion being the weakness of a part of the boiler known as the mud-drum.

Coleman Sellers,	John F. Frazer,
R. E. Rogers,	Henry Morton,
John W. Nystrom,	Samuel J. Cresswell.

Why Bees Work in the Dark.

From the London Chemical News, No. 230.

A lifetime might be spent in investigating the mysteries hidden in a bee-hive, and still half of the secrets would be undiscovered. The

formation of the cell has long been a celebrated problem for the mathematician, whilst the changes which the honey undergoes offer at least an equal interest to the chemist. Every one knows what honey fresh from the comb is like. It is a clear yellow syrup, without a trace of solid sugar in it. Upon straining, however, it gradually assumes a crystalline appearance—it *candies*, as the saying is—and ultimately becomes a solid mass of sugar. It has not been suspected that this change was a photographic action. That the same agent which alters the molecular arrangement of the iodide of silver on the excited collodion plate, and determines the formations of camphor and iodine crystals in a bottle, causes the syrupy honey to assume a crystalline form. This, however, is the case. M. Scheibler has enclosed honey in stoppered flasks, some of which he has kept in perfect darkness, whilst others have been exposed to the light. The invariable result has been that the sunned portion rapidly crystallizes, whilst that kept in the dark has remained perfectly liquid. We now see why bees are so careful to work in perfect darkness, and why they are so careful to obscure the glass windows which are sometimes placed in their hives. The existence of their young depends on the liquidity of the saccharine food persented to them, and if light were allowed access to this the syrup would gradually acquire a more or less solid consistency; it would seal up the cells, and in all probability prove fatal to the inmates of the hives.—*Chronicle of Optics*, "*Quarterly Journal of Science.*"

For the Journal of the Franklin Institute.

Working Steam Expansively. By W. Sewell.

It is a matter of public notoriety that nearly all the screw steamers of the British Navy (and of the British mercantile marine, too, it may be added) use the lap slide-valve and Stephenson link (without an independent cut-off valve,) cutting off the steam between $\frac{5}{8}$ and $\frac{7}{10}$ of the stroke of the piston, that is expanding it through $\frac{3}{8}$ and $\frac{3}{10}$ of the stroke. Of course, with this arrangement the steam can be worked still more expansively at the will of the engineer, by simply raising the link. In this manner the steam can be cut off properly at $\frac{3}{8}$ths the stroke, allowing it to expand through the remaining $\frac{5}{8}$ths. This is precisely similar to the valve-gear now used on all the navy steamers, and almost universally on locomotives; an independent cut-off valve being virtually discarded from all engines running at high speed.

Since economy in the cost of steam power is of as much importance in the British Navy as anywhere else, it is not to be supposed that the British Government adopted this valve-gear, and a point of cut-off at $\frac{5}{8}$ths the stroke of the piston, without satisfying itself that no increase of economy was to be obtained from complicated valve-gear, working independent cut-off valves, and using steam at high grades of expansion. On the contrary, it has experimented extensively, and established practically and incontestibly the practical superiority of the former system.

One of these experiments alluded to was made with the magnificent line-of-battle screw ship "Duke of Wellington," of 3759 tons burthen, and carrying 131 guns. The engines are geared: their cylinders, two in number, have a diameter of 94 inches, and a piston stroke of 54 inches.

With this machinery five experiments were made, cutting off the steam respectively at $\frac{6}{10}$ths, $\frac{1}{4}$th, $\frac{1}{5}$th, $\frac{1}{14}$th, and $\frac{1}{30}$th of the stroke of the piston. The results will be found in the following table:

Cut-off.	Steam in Boilers.	Vacuum.	Throttle.	Revolutions of engine per minute.	Indicator pressure on pistons.	Indicated horse power.	Coal consumed per hour.	Pounds of coal consumed per hour per indicated horse power.
	lbs.	in.	wide		lbs.		cwt.	
$\frac{6}{10}$	13	27	open	30½	18·98	2176·2	70.	7·205
$\frac{1}{4}$	13	27	"	28	13·95	1468·4	45.	6·865
$\frac{1}{5}$	14	27¼	"	26	12.80	1251·1	38.	6·804
$\frac{1}{14}$	14	27¼	"	24	9·30	839·1	26.	6·941
$\frac{1}{30}$	14½	27¾	"	20	8·20	616·5	22.	7·993

EXPLANATION OF INDICATOR DIAGRAMS. *Plate V.*

No. 1.

Mean of the two figures,	18·98 lbs.
Steam,	13
Vacuum,	27
Revolutions,	30½
Consumption of Coal,	70 cwt. per hour.
Throttle-valve wide open. Speed,	10·2 knots.

No. 2. First grade of expansion.

Mean of the two figures,	13·95 lbs.
Steam,	13
Vacuum,	27
Revolutions,	28
Speed of ship,	9 knots.
Consumption of Coal,	45 cwt. per hour.

By losing a trifle over one knot per hour, we saved 25 cwt. of coal in the same time by working on the first grade of expansion.

No. 3. Second grade of expansion.

Mean of the two figures,	12·80 lbs.
Steam,	14
Vacuum,	27¼
Revolutions,	26
Speed of ship,	8½ knots.
Consumption of Coal,	38 cwt. per hour.

No. 4. Third grade of expansion.

Mean of the two figures,	9·30 lbs.
Steam,	14
Vacuum,	27¼
Revolutions,	24
Speed of ship,	7½ knots.
Consumption of Coal,	26 cwt. per hour.

No. 5. Fourth grade of expansion.

Mean of the two figures,	8·2 lbs.
Steam,	14½
Vacuum,	27¾
Revolutions,	20
Speed of ship,	6½ knots.
Consumption of Coal,	22 cwt. per hour.

From the above table it will be seen that the cost of the power in fuel when cutting off at $\frac{1}{8}$ the stroke, (the most economical point in these experiments) was 5½ per cent. less than when cutting off at $\frac{6}{10}$ the stroke, but that when the expansion was carried out to cutting off at $\frac{1}{30}$ the stroke, the cost of power in fuel was 11 per centum more than when cutting off at $\frac{6}{10}$ths. When, however, the conditions of the experiments are examined, the reader will have no difficulty in reaching the conclusion that the $\frac{6}{10}$th point of cutting off was *considerably* the most economical. In the first place, an inspection of the indicator diagrams taken on the occasion, shows that the back pressure with the $\frac{6}{10}$ point of cutting off was greater than with any other point, owing to the smallness of the cylinder ports, which did not allow the larger mass of steam to be evacuated as promptly as the smaller masses with the other points of cutting off. This was purely a fault of proportion in the machine, which alone, if corrected, would have increased the economical effect with the $\frac{6}{10}$ cut-off more than five per cent., and placed it on an equality with the best of the others. Again, the vessel was very short in boiler power, and to maintain the pressure at 13 pounds, the fires had to be forced to the utmost when cutting off at $\frac{6}{10}$; while with the reduced speed of engines and vessel, when cutting off at the shorter points, the combustion of the fuel was comparatively slow, ranging from $\frac{1}{3}$ to $\frac{2}{3}$ only of the maximum. The shorter points of cutting off appear, therefore, in the table, with all the advantage due to slow combustion credited to the expansion, yet notwithstanding all these advantageous circumstances in their favor, the result shows no gain for the expansion. The considerable cost of the power in fuel throughout all these experiments is due to the faulty plan and proportions of boiler employed, the quality of the coal, and the excessive rate of the combustion in the experiment when cutting off at $\frac{6}{10}$, which, as the grate surface was 467½ square feet, reached 33·54 pounds per hour per square foot. In the experiment cutting off at $\frac{6}{10}$, the steam jet was powerfully used to maintain this combustion, and all the steam expended in it counts in the table against that point of cutting off, while in the experiment with the $\frac{1}{8}$th cut-off the rate of combustion (18·21 pounds of coal per square foot of grate per hour,) was maintained by the natural draft. The cut-off valve, except for the $\frac{6}{10}$ point, was an independent adjustable slide; for the $\frac{6}{10}$ point the lap on the main slide was used.

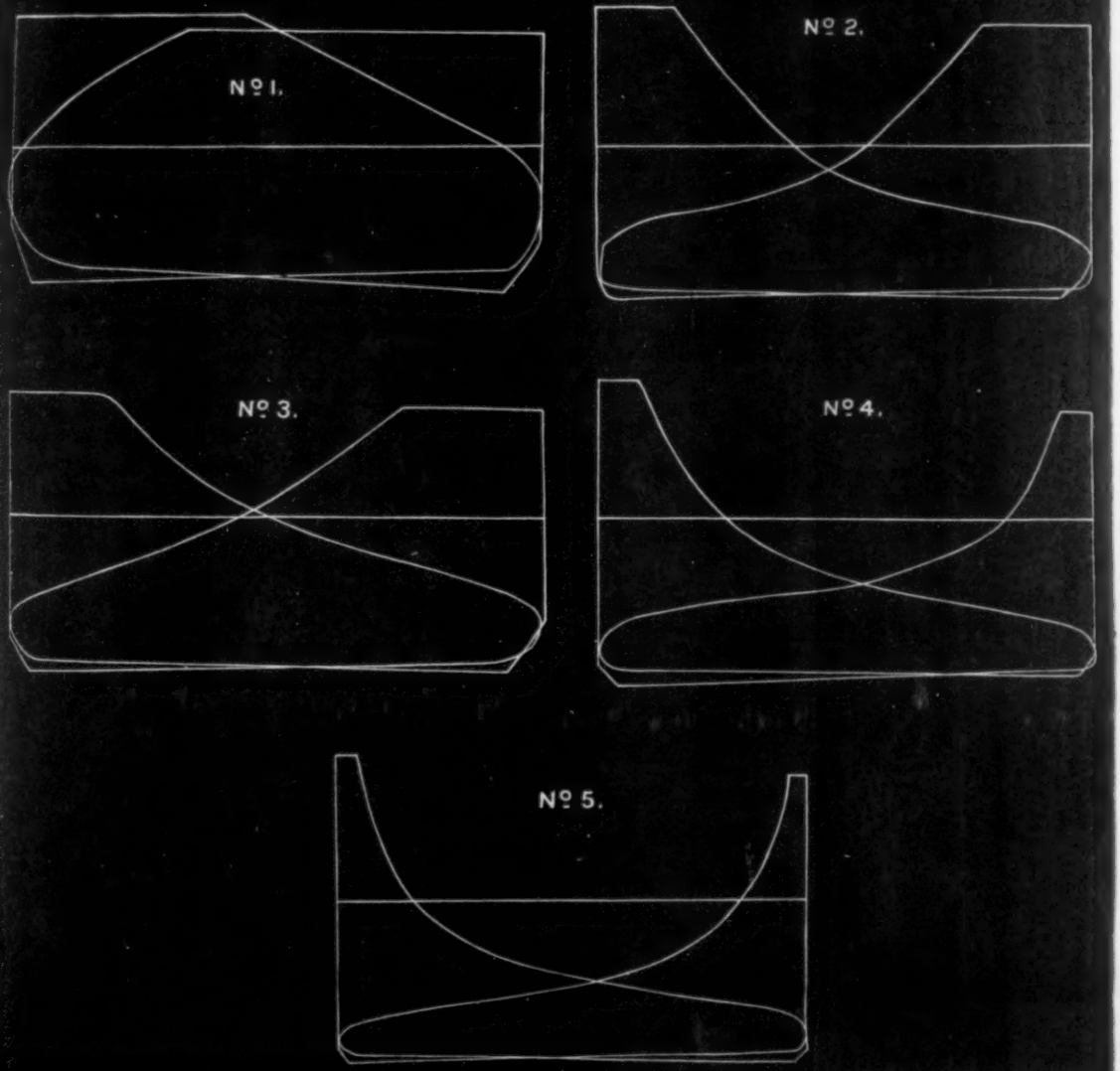
The foregoing information was obtained from Inspector of Machinery, James Steil, R. N., the very able chief engineer of the vessel who conducted the experiments. The results, never before published, are

INDICATOR DIAGRAMS

From the Engines of H.B.M. Steamer Duke of Wellington.

Taken during the Experiments on Expansion.

Scale. 10lbs. to the inch.



remarkably in harmony with those of the numerous, elaborate, and extensive experiments made by our own Navy Department upon the same subject. The intelligent reader is, of course, aware that this question is purely a practical one, to be determined by experiment alone, and that all argument, theories, or hypotheses, are worse than useless—they are mischievous, tending to produce misconception.

It may be as well to add that the maximum speed of the vessel, with the 30½ revolutions of the engines per minute, was 10·2 knots per hour.

Proceedings of the Association for the Prevention of Steam Boiler Explosions, Manchester.

[Report, October 27, 1863.]

From the London Mechanics' Magazine, Nov., 1863.

During the past month there have been examined 300 engines and 440 boilers. Of the latter, 32 have been examined specially, 7 internally, 31 thoroughly, and 370 externally, in addition to which 2 of these boilers have been tested by hydraulic pressure. The following defects have been found in the boilers examined:—Fracture 6 (1 dangerous); corrosion, 18 (2 dangerous); safety-valves out of order, 8; water-gauges ditto, 19 (4 dangerous); pressure-gauges ditto, 14 (1 dangerous); feed apparatus ditto, 5 (1 dangerous); blow-out apparatus ditto, 43; fusible plugs ditto, 4; furnaces out of shape, 5 (2 dangerous); overpressure 3 (3 dangerous); blistered plates, 2. Total 127 (14 dangerous). Boilers without glass water-gauges 11; without pressure-gauges, 2; without blow-out apparatus, 13; without back-pressure valves, 43.

The three furnaces reported above as out of shape, all became so from overheating, consequent upon shortness of water, the injury in each case being observed for the first time, on setting to work in the morning, after the boiler had been standing during the night with the fires banked up, while two of the cases of injury were first observed on a Monday. Each of the boilers had but a single glass water-gauge, two of which were of the pillar construction, while all the boilers were fitted with fusible plugs, which proved inoperative in every instance. One of the boilers was fitted with a fusible plug in each furnace, both the crowns of which came down; the second was fitted with a fusible plug in the right-hand furnace only, and that was the one injured; while the third was fitted with a fusible plug in the left-hand fire-box, and the right-hand was the one injured; but in neither case did the fusible metal melt or give any sign.

There are several ways in which the water supply may run short in a boiler left standing for the night with the fire banked up, all of which should be guarded against.

Where there is only a single glass gauge, the attendant may be deceived as to the actual amount of water in the boiler; and thus the supply be left short overnight, unawares. It is well, therefore, to add

a duplicate-gauge, so that one may act as a check upon the accuracy of the other; this would frequently prevent mistake.

Again, leakage may occur at the feed back-pressure valve, or at the blow-out tap, either from their being in bad condition or imperfectly closed. Engine-men are not sufficiently alive to the importance of examining these as a precaution, but leave them untouched until proved to be actually defective; whereas the feed back-pressure valve should be frequently taken out and its free action ascertained; while the blow-out tap also should be examined, the plug cleaned and greased, as well as ground up if necessary.

A considerable safeguard also to furnace crowns would be found in a low water safety valve, which lets off the pressure of the steam on a deficiency of water occurring within the boiler. It may be true that it would not necessarily prevent the furnace crowns from becoming overheated, since the fire would go on burning if left to itself, after the water supply had run short; but as the valve would not admit of any accumulation of steam, the injury would be confined to the plates over the fire, unless, by excessive carelessness, the attendant should let in the water and get up steam in the morning without observing the distorted furnace crown. Such, however, could not occur without the most culpable blindness, against which it is impossible to provide.

Explosions.—Five boilers not under the inspection of this Association, have exploded during the past month, from which eight persons have been killed and four others injured. Three of the boilers have been personally examined since the explosion.

The particulars of the first of these explosions may be briefly told. The boiler, which was not under the inspection of this Association, was the outer one of three working side by side and connected together, all of them being of the balloon or haystack class.

Each of these boilers was of large diameter, the one in question being about 18 feet at the base, the sides swelling to a still larger diameter at the springing of the hemispherical domed top. The bottom, which was arched, but had no stays to tie it to the crown, was worked into the sides by the plates being bent, and was seated upon a circular kerb of brick-work. Each of the boilers was fitted with a single safety-valve, of rather a rough description, while it was stated that the working pressure had not exceeded 5 lbs., but that the exploded boiler, which was 20 years old, had already been repaired several times.

Immediately over the brick-work seating, the plates for some distance round the boiler had been seriously affected by external corrosion, and reduced in places to one-eighth of an inch in thickness. It was from this cause that the rent occurred, the rent running circumferentially for a distance of 10 or 12 feet parallel with the seating, and for the most part through the body of the plate. A portion of the bottom was blown down into the ash-pit where it remained, the rest of the shell flying to a distance of about 70 feet across a public road adjoining, and carrying away a portion of the boiler-shed in its flight. The engineer, as well as another man lying near to the boiler at the time, were both scalded, the latter dying shortly after in consequence.

Competent inspection could not have failed to detect the dilapidated condition of the boiler, while this explosion shows that, however moderate the pressure may be, no boiler can be safely worked unless frequently submitted to a searching examination.

The second, explosion from which one person was killed, arose from the collapse of the internal flue of a plain Cornish boiler which was not under the inspection of this Association.

The diameter of the shell was about 5 ft. 6 ins., that of the flue 3 ft., the length of both 28 ft., the thickness of the plates generally three-eighths of an inch, but in the flues barely as much, while the steam-pressure was 45 lbs. per sq. inch. The collapse had extended from one end of the flue to the other, the portion over the fire being the least affected, while the remainder behind the bridge was completly flattened down. The boiler had not, however, stirred from its seat, or broken a steam-pipe joint, and all that was needed to resume work with the other two boilers, to which this was connected, was to close the junction valve. A rupture in an externally-fired boiler would have led to the dislocation of the whole series. The damage done by the last explosion of a boiler of that class was nearly £1000.

The cause of the explosion in question was obvious—viz: weakness of the flue. No flue of such dimensions as those just given can be safely worked with steam of a pressure of 45 lbs. on the square inch, unless strengthened either with hoops or flanged seams, or stayed in some other suitable manner. It may be true, however, that some such flues, though unstayed, are working, and have done so for years with steam of an equal or even a greater pressure than the above; still they are continuing to do so only at a risk, and their past immunity from collapse is no security against its occurrence in the future. Some flues gradually work themselves out of the circular shape, and thus become considerably weakened; while all should be placed beyond the suspicion of danger, especially as this can be done at so trifling an expense. For particulars of the best method of adding hoops to those flues which are already made, see the Association's monthly report of June, 1862.

The fireman who was killed was standing in the front of the furnace at the time of the explosion, and blown, rake in hand, to a distance of about 60 feet.

The third explosion occurred to a Cornish boiler with a single flue, internally fired, and not under the inspection of this Association. Fortunately, no one was injured, the explosion occurring during the dinner hour when the men usually working near it were all absent.

The boiler was set on two side walls. It was 23 ft. long; its diameter in the shell was 6 ft.; and in the furnace 3 ft. 6 ins., which was reduced behind the fire bridge; while the thickness of the plates was seven-sixteenths of an inch.

The simple cause of the explosion was external corrosion, the plates having been so eaten away as to be reduced to one-sixteenth of an inch in thickness. Two complete belts, the entire circumference of the boiler, and a plate wide, had severed themselves from it; while a third, which had rent in a somewhat spiral direction, and considerably over-

lapped a single circumference, lay opened out, and still attached to the shell. The two detached belts had fallen, one on each side of the original position of the boiler, doing considerable damage to property thereby; while the main part of it had moved but slightly from its original seating, although it had made a semi-revolution on its longitudinal axis, and lay bottom upwards. The chimney was shattered; the boiler-house, as well as an adjoining building demolished, and some heavy machinery dismantled.

The Chemical Constitution of Pig Iron.

From the London Chemical News, No. 226.

Herr Rammelsberg has recently investigated the views of Karsten and Gurlt on the chemical constitution of pig iron (*see* Erdmann's *Jour. f. pr. Chem.* Bd. 89, S. 393) and has arrived at essentially different conclusions. According to Karsten, white and grey iron are chemical combinations of carbon with iron—the grey iron containing, at the same time, an admixture of graphite. This remains unchanged on the iron being dissolved in acids; while the chemically combined carbon—particularly with concentrated acids—is changed into a strongly odoriferous oil-like combination, the greater part of which volatilizes in the hydrogen, only a small portion dissolving in the acid. According to their views, Spiegeleisen is a chemical combination of iron with the maximum amount of carbon, 5 to 6 per cent., but containing no graphite.

Rammelsberg, on the contrary, has found 1·61 per cent. of graphite in the Müsen Spiegeleisen; and Bromeis, before him, found graphite in the white pig iron of Mägdesprung, as will be seen by the following analysis:—

	Carbon in combination Per cent.	Graphite Per cent.	Total. Per cent.
Bright white pig iron, .	2·518	0·500	3·018
Grey pig iron, . .	2·908	0·550	3·458
Spiegeleisen, . .	3·100	0·72	3·820

As, according to Rammelsberg, the Mädesprung Spiegeleisen only contains 3·9 per cent. of carbon, it seems that this class of iron may contain very unequal quantities of carbon, as well as an admixture of graphite, without any change in its character or properties. It might have been supposed that other metalloids replaced carbon—for example, silicon; but the analyses show, on the contrary, that the iron richest in carbon was also richest in silicon. In the Müsen Spiegeleisen there was found 1·5 per cent. of silicon, in that of Mädesprung 0·17, and in that of Styria from 0·01 to 0·27 per cent.—the latter only containing from 3·75 to 4·14 per cent. of carbon.

According to Gurlt, Spiegeleisen is a fourth carbide, and octahedric

crystalline grey pig iron an eight carbide; the phosphorus, sulphur, and silicon being isomorphous with the carbon, and the manganese with the iron. But if in stating the atomic ratio between the electro-positive and the electro-negative constituents of the Spiegeleisen of Müsen and Mädesprung we adopt Rammelsberg's view, we have a ratio of 1 : 4·5 and 1 : 5·3 instead of 1 : 4. The grey octahedric crystals (the eighth carbide of Gurlt, approximating to wrought iron according to Tunner) are found to be very variable in composition, as shown by the following analysis:—

	From Rothehütte. By Rammelsberg.	From Lauchhammer. By Rammelsberg.	From Gleiwitz. By Gurlt.	From Lolling. By Richter.
Graphite, .	2·604	2·519	2·84	2·122
Carbon, .	0·201	0·373	2·46	0·967
Silicon, .	1·896	1·148	0·26	0·972
Phosphorus, .	0·065	0·406	?	0·021
Sulphur, .	0·069	0·043	?	0·008
Arsenic, .	——	——	——	0·005
Atomic ratio, .	1 : 19	1 : 21	1 : 8	12·5

Therefore, they are generally more than eight atoms of iron to one atom of carbon (silicon, phosphorus).

It will thus be seen that white as well as grey pig iron may crystallize under favorable circumstances, without the formation of the crystals being disturbed by the embedded graphite. The variation in the composition of the crystals, shown by the above analyses—taking place without any alteration in the crystalline form—can only be accounted for by the isomorphism of the elements; and according to Rammelsberg, such is the only possible explanation of the constitution of pig iron. As all the essential constituents of pig iron—such as iron, silicon, phosphorus, and carbon (as diamond)—crystallize on the regular system, and are consequently all isomorphous, we may consider pig iron as an isomorphous mixture of its constituents, which will explain the variation of its composition.

There are many examples of metals belonging to the regular system which form alloys that again crystallize on the same system; but there are also many alloys which crystallize on this system, whose constituent metals belong to distinct crystalline systems. On the other hand, metals that crystallize on the regular system form alloys crystallizing on a different system; thus, for example, silver, zinc, nickel, and copper (iron), alloyed with antimony, crystallize on the pyramidal system. If, therefore, Spiegeleisen is pyramidal, as is probably the case, it belongs to this class of isomorphous mixtures; and the difference of form in grey and white pig iron results from the heteromorphism of their isomorphous constituents. This heteromorphism is evidently a general characteristic of the elements, being just as marked in the metals as is in sulphur and carbon. Doubtless the rhombohedric metals—Sb,

As, Fe, Bi, Zn, Pd, Ir, and Rd,—and the tetragonal Sn, isomorphous to Bo, may, under certain circumstances, crystallize on the regular system, while Au, Ag, Cu, Pb, &c., and also Sn, may be rhombohedric.—*Mining Journal.*

On Boiling Water. By W. GROVE, Esq., Q.C., F.R.S., M.R.I.

From the London Chemical News, No. 225.

A paper by M. Donny ("*Mémoires de l'Académie Royale de Bruxelles*," 1843,) makes known the fact that in proportion as water is deprived of air, the character of its ebullition changes, becoming more and more abrupt, and boiling like sulphuric acid with *soubresauts*, and that between each burst of vapor the water reaches a temperature above its boiling point. To effect this, it is necessary that the water be boiled in a tube with a narrow orifice, through which the vapor issues; if it be boiled in an open vessel it continually re-absorbs air and boils in the ordinary way.

In my experiments on the decomposition of water by heat, I found that with the oxyhydrogen gas given off from ignited platinum plunged into water, there was always a greater or less quantity of nitrogen mixed; this I could never entirely get rid of, and was thus led into a more careful examination of the phenomenon of boiling water, and set before myself this problem—what will be the effect of heat on water, perfectly deprived of air or gas?

Two copper wires were placed parallel to each other through the neck of a Florence cask, so as nearly to touch the bottom; joining the lower ends of these was a fine platinum wire, about an inch and a-half long, and bent horizontally into a curve. Distilled water, which had been well boiled and cooled under the receiver of an air-pump, was poured into this flask so as to fill about one-fourth of its capacity. It was then placed under the receiver of an air pump, and one of the copper wires brought in contact with a metallic plate covering the receiver, the other bent backwards over the neck of the flask and its end made to rest on the pump-plate. By this means, when the terminal wires from a voltaic battery were made to touch the one, the upper and the other lower plate, the platinum wire would be heated, and the boiling continued indefinitely in the vacuum of a very excellent air-pump. The effect was very curious: the water did not boil in the ordinary manner, but at intervals a burst of vapor took place, dashing the water against the sides of the flask, some escaping into the receiver. (There was a projection at the central orifice of the pump-plate to prevent this overflow getting into the exhaustion tube.)

After each sudden burst of vapor, the water became perfectly tranquil, without a symptom of ebullition until the next burst took place. These sudden bursts occurred at measured intervals, so nearly equal in time that, had it not been for the escape from the flask at each burst of a certain portion of water, the apparatus might have served as a time-piece.

This experiment, though instructive, did not definitely answer the

question I had proposed, as I could not, of course, ascertain whether their was some minute residuum of gas which would form the nucleus of each ebullition; and I proceeded with others. A tube of glass, 5 ft. long and $\frac{4}{10}$ths inch internal diameter, was bent into a V-shape; into one end a loop of platinum wire was hermetically sealed with great care, and the portion of it in the interior of the tube was platinized. When the tube had been washed, distilled water which had been well purged of air as before, was poured into it to the depth of 8 inches, and the rest of the tube filled with olive oil; when the V was inverted, the open end of the tube was placed in a vessel of olive oil, so that there would be 8 inches of water resting on the platinum wire, separated from the external air by a column of 4 ft. 4 ins. of oil. The projecting extremities of the platinum wire were now connected with the terminals of a voltaic battery, and the water heated; some air was freed and ascended to the level of the tube—this was made to escape by carefully inverting the tube so as not to let the oil mix with the water—and the experiment continued. After a certain time the boiling assumed a uniform character, not by such sudden bursts as in the Florence flask experiment, but with larger and more distinct bursts of ebullition than in its first boiling.

The object of platinizing the wire was to present more points for the ebullition, and to prevent *soubresauts* as much as possible.

The experiment was continued for many hours, and in some repetitions of it for days. After the boiling had assumed a uniform character, the progress of the vapor was carefully watched, and as each burst of vapor condensed in the oil, which was kept cool, it left a minute bead of gas, ascended through the oil to the bend of the tube: a bubble was formed here which did not seem at all absorbed by the oil. This was analyzed by a eudiometer, which I will presently describe, and proved to be nitrogen. The beads of gas, when viewed through a lens and micrometer scale at the same height in the tube, appeared as nearly as may be of the same size. No bubble of vapor was condensed completely, or without leaving this residual bubble. The experiment was frequently repeated, and continued until the water was so nearly boiled away that the oil, when disturbed by the boiling, nearly touched the platinum wire; here it was necessarily stopped.

To avoid any question about the boiling being by electrical means, similar experiments were made with a tube, without a platinum wire, closed at its extremity, and the boiling was produced by a spirit lamp. The effects were the same, but the experiment was more difficult and imperfect, as the bursts of vapor were more sudden, and the duration of the intervals more irregular.

The beads of gas were extremely minute, just visible to the naked eye, but were made visible to the audience by means of the electric lamp.

In these experiments there was no pure boiling of water—*i.e.*, no rupture of cohesion of the molecules of water itself, but the water was boiled, to use M. Donny's expression, by evaporation against a surface of gas.

It is hardly conceivable that air could penetrate through such a column of oil, the more so as the oil did not perceptibly absorb the nitrogen freed by the boiling water and resting in the bend of the tube; but to meet this conjectural difficulty, the following experiment was made:—A tube, 1 foot long and $\frac{2}{10}$ths inch internal diameter, bent into a slight angle, had a bulb $\frac{3}{4}$-inch diameter blown on it at the angle; this angle was about 3 inches from one end and 9 from the other; a loop of platinum wire was sealed into the shorter leg and the whole tube and bulb filled with and immersed into mercury, water distilled, and purged of air as before was allowed to fill the short leg, and by carefully adjusting the inclination, the water could be boiled so as to allow bubbles to ascend into the bulb and displace the mercury. The effect was the same as with the oil experiment,—no ebullition, without leaving a bead of gas: the gas collected in the bulb, and was cut off by what may be termed a valve of mercury, from the boiling water, then allowed to escape, and so on; the experiment was continued for many days and the bubbles analyzed from time to time; they proved, as before, to be nitrogen; and, as before, continued indefinitely.

A similar experiment was made without the platinum wire, and though, from the greater difficulties, the experiment was not so satisfactory, the result was the same.

As the mercury of the common barometer will keep air out of its vacuum for years, if not for centuries, there could be no absorption here from the external atmosphere, and I think I am fairly entitled to conclude from the above experiments—which I believe went far beyond any that have been recorded—that no one has yet seen the phenomenon of pure water boiling—*i.e.*, of the disruption of the liquid particles of the oxyhydrogen compound so as to produce vapor which will, when condensed, become water, leaving no permanent gas. Possibly, in my experiment of the decomposition of water by ignited platinum, it may be that the sudden application of intense heat, and in some quantity, so forces asunder the molecules, that not having sufficient nitrogen dissolved to supply them with a nucleus for evaporation, the intregal molecules are severed, and the decomposition takes place. If this be so, and it seems to me by no means a far-fetched theory, there is probably no such thing as boiling, properly so called, and the effect of heat on liquid in which there is no dissolved gas may be to decompose them.

Considerations such as these led me to try the effect of boiling on an elementary liquid, and bromine occurred as the most promising one to work upon; as bromine could not be boiled in contact with water, oil, or mercury, the following plan was ultimately devised:—A tube, 4 feet long and $\frac{4}{10}$ths inch diameter, had a platinum loop sealed into one closed extremity; bromine was poured into the tube to the height of 4 inches; the open end of the tube was then drawn out to a fine point by the blow-pipe, leaving a small orifice; the bromine was then heated by a spirit-lamp and when all the air was expelled, and a jet of bromine vapor issued from the point of the tube, it was sealed by the blow-pipe. There was then, when the bromine vapor had condensed

a vacuum in the tube above the bromine. The platinum loop was now heated by a voltaic battery, and the bromine boiled: this was continued for some time, care being taken that the boiling should not be too violent. At the end of a certain period—from half-an-hour to an hour—the platinum loop gave way, being corroded by the bromine; the quantity of this had slightly decreased. On breaking off, under water, the point of the tube, the water mounted, and showed a notable quantity of permanent gas, which on analysis proved to be pure oxygen. As much as a quarter of a cubic inch was collected at one experiment. The platinum wire which had severed at the middle, was covered with a slight black crust, which, suspecting to be carbon, I ignited by a voltaic spark in oxygen in a small tube over lime-water; it seemed to give a slight opalescence to the liquid, but the quantity was so small that the experiment was not relied on. No definite change was perceptible in the bromine; it seemed to be a little darker in color and had a few black specks floating in it, which I judged to be minute portions of the same crust which had formed on the platinum wire, and which had become detached.

The experiment was repeated with chloride of iodine, and with the same result, except that the quantity of oxygen was greater: I collected as much as half a cubic inch in some experiments, from an equal quantity of chloride of iodine; the platinum wire, however, was more quickly acted on than with bromine, and the glass of the tube around it to some extent.

Melted phosphorus was exposed to the heat of the voltaic disruptive discharge by taking this between platinum points in a tube of phosphorus, similarly to an experiment of Davy's, but with better means of experimenting; a considerable quantity of phosphuretted hydrogen was given off, amounting in several experiments to more than a cubic inch.

A similar experiment was made with melted sulphur, and sulphuretted hydrogen was given off, but not in such quantities as the phosphuretted hydrogen. I tried in vain to carry on these experiments beyond a certain point; the substance became pasty, mixed with platinum from the arc, and from the difficulty of working with the same freedom as when they were fresh, the glass tubes were always broken after a certain time. Had I time for working on the subject now, I should use the discharge from the Ruhmkorf coil which had not been invented at the period of these experiments. At a subsequent period when this discharge was taken in the vacuous receiver of an air-pump from a metallic point to a metallic capsule containing phosphorus, a considerable yellow deposit lined the receiver, which, on testing, turned out to be allotropic phosphorus. No gas is however given off. I had an air-pump (described "Phil. Trans.," 1852, p. 101) which enabled me to detect very small quantities of gas, but I could get none. It was in making these experiments that I first detected the striæ in the electric discharge, which have since become a subject of such interesting observations, which are seen, perhaps, more beautifully in this phosphorus vapor than in any other medium, and which cease or become very feeble, where the allotropic phosphorus is not produced.

I tried also phosphorus highly heated by a burning-glass in an atmosphere of nitrogen, but could eliminate no perceptible quantity of gas, though the phosphorus was changed into the allotropic form.

It is not difficult to understand why gas is not perceptibly eliminated in the last two experiments; the effect is probably similar to that described in my paper on the "*Decomposition of Water by Heat*," where when the arc or electric spark is taken in aqueous vapor, a minute bubble of oxyhydrogen gas is freed and disseminated through the vapor, re-combination being probably prevented by this dilution; but, however long the experiment may be continued, no increased quantity of the gas is obtained, all beyond this minute quantity being re-combined. If, however, the bubble of gas be collected, by allowing the vapor to cool, and then expelled, a fresh portion is decomposed, and so on.

So with the phosphorus in the experiments in the air-pump and with the burning-glass; if any gas is liberated it is probably immediately re-combined with the phosphorus; possibly a minute residuum might escape re-combination, but the circumstances of the experiment did not admit of this being collected, as the gas was with the aqueous vapor.

When, on the other hand, the gas freed is immediately cut off from the source of heat, as when the spark is taken in liquids, an indefinite quantity can be obtained.

Decomposition and the elimination of gas may thus take place by the application of intense heat to a point in a liquid, or also in gas or vapors; but in the latter case it is more likely to be masked by the quantity of gas or vapor through which it is disseminated.

I believe there are very few gases in which some alteration does not take place by the application of the intense heat of the voltaic arc or electric spark. If the arc be taken between the platinum points in dry oxygen gas over mercury, the gas diminishes indefinitely, until the mercury rises, and by reaching the point where the arc takes place, puts an end to the experiment. I have caused as much as a cubic inch of oxygen to disappear by this means. I at one time thought this was due to the oxidation of the platinum; but the high heat renders this improbable, and the deposit formed on the interior of the glass tube in which the experiment is made has all the properties of platinum-black; so if the spark from a Ruhmkorf coil be taken in the vapor of water for several days, a portion of gas is freed which is pure hydrogen, the oxygen freed being probably changed into ozone, and dissolved by the water in this case, while in the former it combined with the mercury.

I have alluded to the eudiometer by which I analyzed the gases obtained in these experiments; it was formed simply of a tube of glass, frequently not above $2\frac{1}{2}$ millimetres in diameter, with a loop of wire hermetically sealed into one end, the other having an open bell mouth. By a platinum wire a small bubble of the gas to be examined could be got up through water or mercury into the closed end of the tube, and by the addition of a bubble of oxygen or hydrogen gas, a very accurate analysis of very minute quantities of gas could be made: I have analyzed by this means quantities no larger than a partridge-shot.

I need hardly allude to results on the compound liquids, such as oils

and hydrocarbons, as the fact that permanent gas is given off in boiling such liquids would not be unexpected; but the above experiments seem to show that boiling is by no means necessarily the phenomenon that has generally been supposed, viz: a separation of cohesion in the molecules of a liquid from distention by heat. I believe, from the close investigation I made into the subject, that (except with the metals, on which there is no evidence) no one has seen the phenomenon of pure boiling without permanent gas being freed, and that what is ordinarily termed boiling arises from the extrication of a bubble of permanent gas either by chemical decomposition of the liquid, or by the separation of some gas associated in minute quantity with the liquid, and from which human means have hitherto failed to purge it; this bubble once extricated, the vapor of the liquid expands it, or, to use the appropriate phrase of M. Donny, the liquid evaporates against the surface of the gas.

My experiments are, in a certain sense, the complement of his. He showed that the temperature of the boiling point was raised in some proportion as water was deprived of air and that under such circumstances the boiling took place by *soubresauts*. I have, I trust, shown that when the vapor liberated by boiling is allowed to condense, it does not altogether collapse into a liquid, but leaves a residual bubble of permanent gas, and that at a certain point this evolution becomes uniform.

Boiling, then, is not the result of merely raising a liquid to a given temperature; it is something much more complex.

One might suppose that with a compound liquid the initial bubble by which evaporation is enabled to take place, might, if all foreign gas were or could be extracted, be formed by decomposition of the liquid: but this could not be the case with an elementary liquid; whence, the oxygen from bromine or the hydrogen from phosphorus and sulphur?

As with the nitrogen in water, it may be that a minute portion of oxygen, hydrogen, or of water, is inseparable from these substances, and that if boiled away to absolute dryness, a minute portion of gas would be left for each ebullition.

With water there seems a point at which the temperature of ebullition and the quantity of nitrogen yielded become uniform, though the latter is excessively minute.

The circumstances of the experiments with bromine, phosphorus, and sulphur did not permit me to push the experiment so far as was done with water; but as far as it went the result was similar.

When an intense heat, such as that from the electric spark or voltaic arc is applied to permanent gas, there are, in the greater number of cases, signs either of chemical decomposition or of molecular change; thus compound gases, such as hydrocarbons, ammonia, the oxides of nitrogen, and many others are decomposed. Phosphorus in vapor is changed to allotropic phosphorus, oxygen to ozone, which, according to present experience, may be viewed as allotropic oxygen. There may be many cases where, as with aqueous vapor, a small portion only is decomposed, and this may be so masked by the volume of undecomposed gas as to escape detection. If, for instance, the vapor of water were uncondensible, the fact that a portion of it is decom-

posed by the electric spark or ignited platinum would not have been observed.

All these facts show that the effect of intense heat applied to liquids and gases is much less simple, and presents greater interest to the chemist, than has generally been supposed. In far the greater number of cases, possibly in all, it is not mere expansion into vapor which is produced by intense heat, but there is a chemical or molecular change. Had circumstances permitted, I should have carried these experiments further, and endeavored to find *experimentum crucis* on the subject. There are difficulties with such substances as bromine, phosphorus, &c., arising from their action on the substances used to contain and heat them, which are not easy to vanquish, and those who may feel inclined to repeat my experiments will find these difficulties greater than they appear in narration; but I do not think they are insuperable, and hope that, in the hands of those who are fortunate enough to have time at their disposal, they may be overcome.

To completely isolate a substance from the surrounding air, and yet be able to experiment on it, is far more difficult than is generally supposed. The air-pump is but a rude mode for such experiments as are here detailed.

Caoutchouc joints are out of the question. Even platinum wires carefully sealed into glass, though, as far as I have been able to observe, forming a joint which will not allow gas to pass, yet it is one through which liquids will effect a passage, at all events, when the wires are repeatedly heated.

In some experiments with the ignited platinum wire hermetically sealed into a tube of glass, the end of the tube containing the platinum wire was placed in a larger tube of oil, to lessen the risk of cracking the glass. After some days experimenting, though the sealing remained perfect, a slight portion of carbon was found in the interior liquid. This does not affect the result of my experiments, as I repeated them with glass tubes closed at the end and platinum wires, and also without the oil-bath: but it shows how difficult it is to exclude sources of error. When water has been deprived of air to the greatest practicable extent it becomes very avid for air. The following experiment is an instance of this:—A single pair of the gas battery, the liquid in which was cut off from the external air by a greased glass stopper, having one tube filled with water, the other with hydrogen; the platinized platinum plates in each of these tubes were connected with a galvanometer, and a deflection took place from the reaction of the hydrogen on the air dissolved in the water. After a time the deflection abated, and the needle returned to zero, all the oxygen of the air having become combined with the hydrogen. If now the stopper were taken out, a deflection of the galvanometric needle immediately took place, showing that the air rapidly enters the water as water would a sponge. Absolute chemical purity in the ingredients is a matter, for refined experiments, almost unattainable. The more delicate the test, the more some minute residual product is detected. It would seem (to put the proposition in a somewhat exaggerated form)

that in nature everything is to be found in anything if we carefully look for it.

I have indicated the above sources of error to show the close pursuit that is necessary when looking for these minute residual phenomena. Enough has, I trust, been shown in the above experiments to lead to the conclusion that hitherto simple boiling, in the sense of a liquid being expanded by heat into its vapor, without being decomposed or having permanent gas eliminated from it, is a thing unknown. Whether such boiling *can* take place may be regarded as an open question though I incline to think it cannot; that if water, for instance, could be absolutely deprived of nitrogen, it would not boil until some portion of it was decomposed; that the physical severance of the molecules by heat is also a chemical severance. If there be anything in this theoretic view, there is great promise of important results on elementary liquids, if the difficulties to which I have alluded can be got over.

The constant appearance of nitrogen in water, when boiled off out of contact with the air almost to the last drop, is a matter well worthy of investigation. I will not speculate on what possible chemical connexion there may be between air and water. The preponderance of these two substances on the surface of our planet, and the probability that nitrogen is not the inert diluent in respiration that is generally supposed, might give rise to not irrational conjectures on some unknown bond between air and water. But it would be rash to announce any theory on such a subject; better to test any guess one may make by experiment than to mislead by theory without sufficient data, or to lessen the value of facts by connecting them with erroneous hypotheses.

Preservation of Meat.

From the Journal of the Society of Arts, No. 590.

The preservation of meat, whether for the use of our sailors on board ship, or for other purposes, has long engaged the attention of scientific and practical men, and various ways of affecting this object have been from time to time devised. The methods hitherto adopted on a large scale have been the packing of cooked meat in air-tight cases, or impregnating it with salt, and keeping it in barrels immersed in brine. The first, though effectual for preserving the meat for almost any amount of time, leaves the flesh, even when the utmost care is taken in the process, more or less insipid and tasteless; the second, though also preservative for a considerable time, renders the meat not only flavorless, but absolutely extracts from it, as Liebig tells us, nearly all its nutritive properties, as well as those peculiar qualities which are necessary for keeping the body in health. It is well-known that a long continuance of such food, thus prepared, engenders scurvy. The Admiralty are now making experiments with a process devised by Dr. Morgan, an Irish gentleman; and a few weeks since some animals were slaughtered, and their carcases subjected to this process in the presence of officers of her Majesty's Victualling Department at Deptford. A bullock having been killed in the usual way, the chest was immediately

opened, and a metal pipe with a stop-cock inserted in connexion with the arterial system. This pipe was connected by means of elastic tubing, with a tub filled with brine placed at an elevation of about twenty feet above the floor. The stop-cock being turned, the brine forced itself through the arteries of the animal, and, passing through the capillaries, flowed back through the veins, carrying with it all the blood, making its exit by means of an incision provided for that purpose. About six gallons of brine passed thus through the body, washing out blood from the vessels. Having thus cleared all the vessels, the metal pipe was connected with another tube similarly placed, containing the preservative material to be injected, and at the same time their exit, after traversing the body, was prevented. On communication being made, the liquid became forced into the vessels, and, by means of the pressure, it penetrated into every part of the animal, and might be seen exuding at any point where an incision was made. The liquid used on the occasion of the late experiments consisted of six gallons and a half of brine, 10 lbs. of sugar, ¾-lb. of saltpetre, with half a bottle of catchup, and an infusion of cloves and pepper. The whole process is very rapid, and is extremely simple, requiring nothing that can be termed machinery. It took no longer than three minutes to send the first six gallons of liquid through the animal to wash out the vessels, and about three minutes more to inject the animal with the preservative liquid. Indeed, so rapid is the whole proceeding, that, even on the occasion above referred to, where the men were unaccustomed to the work, and the arrangements were necessarily imperfect, the time occupied was only twenty minutes from the killing the animal to the complete infiltration being made. The beast is then skinned, cut up into pieces, large or small, as may be required, and taken to a drying room, where it is hung until thoroughly dried, after which it is packed in boxes with sawdust and charcoal. It is confidently believed that the meat treated in this manner will stand any climate, and the flesh is free from the insipidity of that ordinarily preserved in tins, and its goodness is not destroyed by having nearly all its valuable properties drawn out by immersion in brine. So far as its preservative powers have been tested in this country, the process is stated to answer the purpose. A purveyor at Portsmouth has for some time past treated meat in this way with success, and sells it in the regular course of trade. It is obvious that any variety of liquids, chemicals, or condiments may be thus injected into the animal, and the meat flavored in any way that may be thought desirable; the meat may also be dried or cured like ham or bacon, if so wished. Indeed, it would seem that the method is peculiarly fitted for this purpose. In hot countries, and in countries where animals are abundant, and where now they are bred almost entirely for their wool, fat, and hides, the process seems especially valuable, as by it the meat, instead of being thrown away, might be rendered available for export for food to other countries. The Victualling Department have had a few animals thus prepared for experiment, and it is intended to send the meat out on voyages to various parts of the world to test its keeping qualities. So little machinery is required,

that a ship's crew could readily carry out the process at any place where they could land and animals were abundant, and thus lay in a store of meat which, although, no doubt, salted to a certain extent, would not have the same disadvantages in a sanitary point of view as meat preserved in brine-pickle. Some lengths of india-rubber tubing, pieces of metal tube with stop-cocks, and tubs for holding the liquid, are all that is required. In hot climates the drying may be effected in the open air, and in other cases there would be no difficulty in arranging a room for the purpose, either on shore or on board ship.

Description of a New Machine for drawing Solid Metal Tubing.

From the Journal of the Society of Arts, No. 589.

A new machine for drawing solid metal tubes has lately been tested in London with considerable success. The system was originally introduced into England by a French gentleman, about ten years ago, but in consequence of defective mechanical arrangements was at that time unsuccessful. The Stephenson Tube Company have lately erected extensive works at Birmingham for the manufacture of brass and copper tubes on this principle, but the new machine is especially adapted to the formation of tubes of steel, and other hard and close-grained metals. The ordinary method of forming wrought iron tubes is by bending round a long narrow plate of the metal so that the edges meet, and then having reduced them to a welding heat, to join them together by drawing the tube through a die made for that purpose. The machine in question entirely differs from this, and its action may be described as follows:—It consists of two large cast iron cylinders, 11 feet long, placed opposite each other, and connected by a ram of 10 feet travel, which is driven out of the one and into the other alternately. On each end of the cylinders is a massive flange, pierced with eight holes, some four inches across; on the ram is a similar flange, but double, offering eight holes to each cylinder. The diameter of this hydraulic ram is 16½ inches, and the force obtainable upwards of 600 tons. A draw-plate or die of peculiar form is placed in one of the holes in an inner flange of one of the cylinders, and the tube to be drawn slightly tapered at the end, to allow of its passing into the die, is placed therein, having previously received a steel-headed mandril, intended to act on the interior of the tube. The tube is fixed on the flange of the ram by means of a screw pin projecting through the flange of the cylinder, and the stem of the mandril is fastened to the further end of the cylinder. The water being forced into the cylinder the ram is driven forward, and the tube is drawn bodily through the die, or draw-plate, and over the mandril, the head of which, inside the tube, is placed within the circle of contact of the die outside. By this means the tube is drawn externally and internally at the same time, and an eighth of an inch of steel may, at a time, be displaced from the surface, the tube being elongated proportionately; but in practice it is found more convenient to take off less at each pass in order to avoid fatiguing the metal overmuch. It is stated that two bars of rough iron, as delivered from the forge, 3½ inches diameter and 4 feet long, were placed

in the machine and drawn through the die, the drawing occupying some five minutes. The bars issued from the die with a surface finer than can be obtained at present by any known mechanical means, except continued friction, and infinitely truer than any turning-lathe produces. It would take a man at a lathe two days at least to turn these bars, and he would then produce inferior results to those obtained in perhaps a quarter of an hour, if the time employed to fix and unfix the bars in the hydraulic drawing bench be included. It will scarcely be necessary to remark, in conclusion, that should this machine fulfil the expectations of the inventor, it will cause a complete revolution in the manufacture of gun-barrels, hollow shafting, axles, piston-rods, and such-like articles.

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*To make an Absolutely Correct Camera copy of a Chart by Means of a Single Distorting Lens.** By ROBERT H. BOW, C. E.

From the Lond. Civ. Eng. and Arch. Jour., December, 1863.

I have already mentioned before this society that distortion in a negative may be corrected in the positive taken from it by the camera, whether the distortion arises from the defects of the lens, or from the camera not having been placed squarely to the object in taking the negative. This short paper is merely to call attention to a very simple practical application of the method of correcting that distortion which arises from the defects of the lens.

I have here a camera with an achromatic plano-convex lens of 4½ ins. focal length. It produces pictures which, if of moderate size, are free from anything that a casual observer would detect as distortion; but the following table will give you an idea of the real amount thereof. Before you I have laid a negative taken by the same lens, embracing a field of 80°.

Table showing the Distortion of Scale (measured in the direction of a diameter of the picture) when a distant† view is taken with the plano-convex achromatic lens of four and a half inches focal length, described at page 241 of the current volume of *The Civil Engineer and Architect's Journal*, the stop being three-quarters of an inch from the flat side. (The points in the image corresponding with the pencils at the several angles of obliquity, are obtained by the method referred to in my paper on the "Curvatures of the Image.")

	Intervals measured (approximately) from the actual image.	Intervals according to calculation when there is no distortion.
Between rays at 0° and 5°	100	100·00
" " 5° " 10°	101	101·54
" " 10° " 15°	102	104·73
" " 15° " 20°	103	109·75
" " 20° " 25°	105	116·97
" " 25° " 30°	107	126·93
" " 30° " 35°	110	140·42

From this table it appears, then, that an object lying between 30° and 35° of obliquity will suffer a contraction from 140·42 to 110, or a diminution of 30·42 upon a length of 140·42, which is equivalent to

* Read at a meeting of the Edinburgh Photographic Society, 21st October, 1863.

† When the object is not so distant, the distortion will be somewhat greater.

21·7 per cent. as compared with the size it would appear if seen lying within the 5° of obliquity. Of course this is an extreme obliquity, but it is the accompanying distortion which chiefly interferes with our using such an angle, as it may be observed that the negative exhibited is sufficiently sharp for many purposes up to that extent, or for a field embracing 70°.

Between the obliquities of 20° and 25° the distortion is equivalent to a contraction of 117 into 105, or a reduction of 12 on a length of 117, which amounts to a diminution of 10·3 per cent. compared with the scale at the central intervals. Now even this will be allowed by all to be serious; for it is as though a foot measure, given correctly near the centre, measured only 10·8 inches near the side of the picture.

Let us now proceed to explain the method of copying without any distortion, whatever the field embraced:—

In the figures let O represent the object or chart, *P* the photograph, and *c* the lens—the line beside *c* indicating the position of the stop.

Fig. 1.

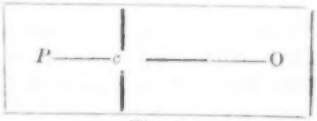

Fig. 2.

Fig. 3.

In Fig. 1, *P* represents the negative first obtained with the stop, arranged as usual for views, and *P* will therefore be "barrelled." In Fig. 2, if the chart were placed at O, we should get a distorted negative *P* of the "pincushion" order. But if instead of a straight object O, in Fig. 2, we place the barrelled negative *P*, as in Fig. 3, we get a copy at P, free from all distortion, if made of the same size as O, since the tendency of the arrangement to produce pincushioning then just suffices to correct the barrelling. In fact each pencil of light is sent back from the negative *P*, in Fig. 3, to the final copy *P*, in exactly the same course as that by which it arrived in the first instance from O, in Fig. 1 (except in so far as the focussing may be defective.)

This absolutely perfect correction of the distortion only takes place when the final copy is made of the same size with the original chart. But practically, if no great difference exists between the lengths of *P c* in Figs. 1 and 3, the correction may be assumed as sufficiently perfect; and in order that these distances may not vary much, we should make our negative considerably less than the final copy to be made from it, when the final copy is to differ much in scale from the chart.

If the negative be made of intermediate size between the chart and the final copy, and the latter be taken with the stop on its side of the lens, the correction of the barrelling will be overdone, so that the final copy will be slightly pincushioned, but generally so slightly that this mode of proceeding will frequently be preferred when a very sharp result is desired.

If it be desired that the final photograph P, in Fig. 3, should be a negative, we may effect this by substituting for P a transparency printed from the first negative by superposition.

Coal and Iron in France and other Countries.

From the Journal of the Society of Arts, No. 590.

A work on property in minerals, by M. Edouard Dalloz, was presented a few days since to the Academy of Moral and Political Sciences of Paris, by M. Wolowski, who read a report on the book in question and on various matters connected with the subject on which it treated. M. Dalloz treats especially of mineral legislation in France and Belgium, but touches incidentally on that of other nations, and also upon the statistics and economy of the mineral question in general, its bearings and influences. In connexion with the statistical portion of the question, M. Wolowski was able to avail himself of more recent records than those used by M. Dalloz, and made an elaborate report to the Academy, from which the following is extracted:—In 1789 France used only half a million tons of coal a year, and of this little more than one-half was raised at home; but in 1830 the production had grown to 1,800,000, and the quantity imported to 600,000 tons. The progress made since that time is as follows:—

	Production. Tons.	Importation. Tons.	Consumption. Tons.
1835,	2,500,000	800,000	3,300,000
1840,	3,000,000	1,290,000	4,290,000
1845,	4,000,000	2,200,000	6,200,000
1847,	5,400,000	2,500,000	7,600,000

During the four following years political events stopped the growth, both of production and consumption; the quantity raised in France fell back to four millions of tons, and the imports to little more than two millions. Since 1852 the progress was great and constant—

1853,	5,900,000	3,500,000	9,400,000
1854,	6,000,000	4,000,000	10,000,000
1855,	7,400,000	5,000,000	12,400,000
1856,	7,900,000	5,000,000	12,900,000
1857,	7,900,000	5,195,000	13,095,000
1858,	7,353,000	4,840,000	12,200,000
1859,	7,483,000	4,900,000	12,400,000

In 1860 came the change in the tariff, and the following are the results to the present time:—

1860,	8,391,000	5,200,000	13,600,000
1861,	8,400,000	5,300,000	13,700,000
1862,	9,400,000	5,200,000	14,600,000
1863,	10,000,000	5,200,000 more than	15,000,000

The return for last year being an approximate estimate made by the Imperial Administration of Mines. By these figures it will be seen that France raises for herself at present as much as she consumed in the whole of 1854; and that the consumption itself has doubled since 1847. It will be observed also, a fact not noted in the report, that since 1860

there has scarcely been any increase in the imports, while the home production has grown about 20 per cent. Side by side with these facts M. Wolowski drew special attention to the fact that the quantity of coal brought to the surface in England had grown from twenty-six millions of tons in 1837, to eighty-four millions in 1862, and that she produced far more than the half of all the coal raised in the world. The produce of America was given at fifteen, that of Belgium at nine, and of Germany at upwards of fourteen millions of tons per annum. As regards iron, the whole of the furnaces in France only produced 69,000 tons of pig iron in 1789, 115,000 tons in 1812, and 112,500 tons in 1819. Since that time there has been a great increase, and it is important, says M. Wolowski, to see what has been the effect of the commercial treaty of 1860. In 1830 the quantity of pig iron had risen to 347,773 tons, and that of wrought iron to 237,379 tons, and in 1847 it had grown to 602,772 tons of the former, 376,686 of the latter. In 1848 the totals had fallen off to the extent of 50 per cent. No reaction occurred till 1854; but in 1859 the totals were 856,000 and 520,000 respectively, rather less than those of 1858, and considerably below those of the previous year. During the years 1860-61 a great increase took place, and in 1862 the amount of pig iron produced rose to 1,053,000 tons, while that of 1863 is estimated at 1,180,000 tons. Of the last total, says M. Wolowski, 280,000 tons were charcoal iron, the remaining 900,000 tons having been produced with the aid of coal alone or mixed. As regards wrought iron the report gives the following return for the year 1863:—

Charcoal iron,	76,800 Tons.
Coal iron,	606,000 "
Mixed fuel,	22,700 "
Total,	705,000 "

The production of charcoal iron has fallen in the last three years from 90,655 to 76,800 tons, while that of coal iron has risen from four to six hundred thousand tons. In 1847, more than half the iron produced in France was charcoal iron. The total product of all Europe in 1808, according to M. Héron de Villefosse, was only 825,000 tons of pig iron, while the quantity now made is about seven millions of tons, of which England produces nearly four; and in 1862 France 1,180,000; Germany, 591,593; Sweden, 143,000; Austria 270,000; and Belgium, 161,000 tons. After such totals as these it is curious to note that in 1740 England made none but charcoal iron, and only produced 17,500 tons in the year.

Description of a New Gun invented by James Mackay.

From the Lond. Mechanics' Magazine, April, 1864.

Two or three weeks ago the world heard for the first time of the Mackay gun. Yet, short as this space is, it has been quite long enough to demonstrate that Mr. Whitworth and Sir W. Armstrong must indeed look to their laurels. Mr. James Mackay is a Liverpool timber merchant, and the gun which will in future be known by his name has, we

believe, been constructed wholly at his own expense, certainly without Government aid. The principles involved in its construction are novel, elegant, and ingenious. Hitherto we have learned to regard windage as a thing to be avoided by the use of every expedient at our command, because it wastes power and leads to inaccuracy of flight in the projectile. Mr. Mackay, on the other hand, turns this same windage to account, and makes it the motive power for spinning his projectiles round. With a smooth bore, it is evident that the gas generated would make its escape quicker than the heavy shot. To prevent this, spiral grooves of much greater acuteness than those employed in the ordinary rifled cannon run through the interior of the Mackay gun, and as the gas follows the course of these grooves, it has to travel about six feet further than the shot before the muzzle is reached. It is thus prevented from escaping before the shot, which is whirled swiftly round by the revolving current of gas that follows close upon and grasps it, as it were, in its embrace, during its passage through the chase of the gun. The rotary motion is thus imparted to the shot solely by the friction of the escaping gases on its smooth sides, the gun having in all respects, save these spiral grooves—which must not be confounded with rifling proper—the character of a smooth bore, as the shot rests, upon the raised surfaces of the grooves, by which its motion is not impeded. Thus the initial velocity of the smooth bore is obtained, as well as the rotary motion necessary to precision; and considering the importance of high velocities as exemplified in the effectiveness of the old 68-pounder at short distances, it is a great deal in favor of the Mackay gun that it discharges its shot with a momentum of some 300 or 400 feet more per second than does the Armstrong gun. The projectiles used are cylindrical steel bolts with a conical head. They are balanced from the centre, and the smaller weight at the cone end is compensated for by scooping out a portion of the metal at the other end, and they are perfectly smooth.

The experimental gun has been manufactured of wrought iron by the Mersey Steel and Iron Works Company. It weighs 9 tons, has a bore of 8·12 inches, and in other respects corresponds with the general features of the ordinary 68-pounder. There are 12 grooves, and, as the shot do not enter these grooves, it allows of a much sharper twist than in ordinary rifled guns. The velocity has been found to be 1640 feet a second; the utmost range has not yet been tested. Messrs. Laird and Company, of Birkenhead, are now building an armor-plated vessel called the Agincourt, for the Government, and it was agreed to test the gun against a section of the side of that vessel. It was intended to make the trial last Saturday, but unfavorable weather prevented its being proceeded with. On Monday morning, however, the experiments commenced. The target consisted of an outer plate, 7 feet square and 5½ inches thick, of rolled iron; next came 9 inches of teak, then an inner plate or skin three-fourths of an inch thick, then angle-iron and ribbing, and finally a backing up with timber balks and supports 18 inches thich. The plates were stated, on competent authority, to be the best that can be made of rolled iron. Messrs. Laird

supplied the whole, and expressed confidence in the strength of the target. The gun was fired in the presence of Mr. James Mackay, Mr. D. Mackay, Mr. Bouck, and other gentlemen, shortly after 8 o'clock in the morning. There is a little novelty in the mode of loading. Between the powder and the shot a bag of sawdust is placed, Mr. Mackay's object being to give, by means of this substance, a slight movement to the shot before the whole of the powder is ignited. This, without diminishing the force of the charge, prevents a heavy concussion, and renders the danger of bursting considerably less; so much so, that the inventor expresses his confidence that a charge of 100 pounds of powder would not damage the gun. The recoil of the gun, as well as the report is comparatively small. On Monday the gun was charged with 30 pounds of powder and a cast steel shot, weighing 167 pounds, made by Messrs. Thomas Firth and Son, of Sheffield. The range was 200 yards. The shot struck the target with a dull thud a little below the bull's eye on the right, and in the very strongest part, where it was backed up by the rib of the ship's side, the angle-iron, and the timber balk. At the point of impact a perfectly circular hole was cut. The shot then powdered the teak, passed through the inner skin and the angle-iron, shattered the timber balk into fragments, and was picked up 82 yards beyond the target, together with a circular piece of iron armor, about 80 pounds weight, which it had carried with it through the back supports. The sand showed that it had spun to the last. About 70 fragments of iron, bolts, and fragments of the inner skin and angle-iron were picked up 100 yards from the target. The shot when found was reduced from 13 inches to 11 inches in length, and increased about an inch-and-a-half in diameter at the end which struck the target. The other end was uninjured. Tho whole target was forced back about 6 inches, and so much deranged that more shots were not fired.

On Radiation through the Earth's Atmosphere. By JOHN TYNDALL, Esq., F.R.S. Prof. Nat. Phil., Royal Institution.

Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research, can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to

be able to affix definite physical ideas, both to the term atmosphere and the term radiation It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent. of atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapor. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapor, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns, and of all stars, is in reality a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of Radiant Heat; and, if competent to excite the nerves of vision, we call it Light.

Aqueous vapor was defined to be an invisible gas. Vapor was permitted to issue horizontally with considerable force from a tube connected with a small boiler. This track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapor, but vapor condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapor. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapor was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snow-ball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapor within, which became beautifully illu-

minated by the beam; while, upon a screen behind, a richly colored halo due to diffraction by the little cloud within the receiver flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapor. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapor molecules would be an open door for the passage of the undulations; and that if those waves were at all intercepted, it would be by the substances which form 99½ per cent. of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapor intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapor. The vapor was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapor of the air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapor. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen, with a single atom of aqueous vapor, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition a discovery, if it be worth the name, emerges with its fibre strengthened; as the human character gathers force from the healthy antagonisms of active life. It was urged, that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it without ascribing so enormous a comparative action to aqueous vapor. For example, the cylinder which contained the air in which these experiments were made, was stopped at its ends by plates of rock-salt, on account of their transparency to radiant heat. Rock-salt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rock-salt; and it is well-known that brine is very impervious to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube, for a moment on the salt; brilliant colors of thin plates (soap-bubble colors) flashed forth immediately upon the screen—these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the

cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absorption of aqueous vapor.

This objection was met in two ways. First, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results, in a cylinder open at both ends.

It was next surmised, that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapor of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapor was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapor diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapor an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapor so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day on the driest portion of the earth's surface would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapor present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rock-salt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapor was here manifest, as in all the other cases.

No doubt, therefore can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is

perfectly certain that more than ten per cent. of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly discovered property of aqueous vapors must exert on the phenomena of meteorology.

This aqueous vapor is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer night the aqueous vapor from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapor constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapors of the equatorial ocean; they rise, but for a time a vapor screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space; and when, by their levity, they have penetrated the vapor screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapor is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapor will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapor by its own radiation must play a most important part. The rain quits the ocean as vapor; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapor, rises in columns, so as to penetrate the vapor screen which hugs the earth; in the presence of space, the head of each pillar wastes its heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution by reference to the radiant and absorbent properties of aqueous vapor. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different

it is charged with aqueous vapor, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly discovered property of transparent aqueous vapor. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

Proc. Royal Ins., Jan. 23, 1863.

Application by Professor Pepper and another for an injunction to restrain Myers and another from performing the Ghost Illusion.

From the London Mechanics' Magazine, November, 1863.

This was an application for an injunction to restrain the defendant from performing the celebrated "Ghost" illusion. The plaintiffs were Professor Pepper and Joseph Heath, of Liverpool, and the circumstances of the case were as follows (according to the plaintiffs' version): —Mr. Pepper, with one Henry Dircks, were the first inventors of "improvements in apparatus to be used in the exhibition of dramatic and other like performances," and in August, 1863, letters patent were granted, giving them exclusive right of using their invention for 14 years. The method by which the illusion is produced is as follows:—The nature and object of the invention is, by a peculiar arrangement of apparatus, to associate on the same stage a phantom or phantoms with a living actor or actors, so that the two may act in concert. The arrangement of the theatre requires, in addition to the ordinary stage, a second one at a lower level, hidden from the audience as far as direct vision is concerned; the hidden stage is to be strongly illuminated by artificial light, and capable of being darkened instantaneously, whilst the ordinary stage and the theatre remain illuminated by ordinary lighting. A large glass screen is placed on the ordinary stage and in front of the hidden one. The spectators will thus not observe the glass screen, but will see the actors on the ordinary stage through it as if they were not there; nevertheless, the glass will serve to reflect to them an image of the actors on the hidden stage when these are illuminated, but this image is made immediately to disappear by darkening the hidden stage. The glass screen is set in a frame, so that it can be readily moved to the place required, and it is to be set at an inclination to enable the spectators, whether in the pit, boxes, or gallery, to see the reflected image. The glass is adjustable, and it is readily adjusted to the proper inclination by having a person in the pit and another in the gallery to inform the party who is adjusting the glass when they see the image correctly. The hidden stage is capable of being closed at the top by trap-doors. When the trap-doors are closed actors on the ordinary or visible stage can pass freely to and fro above the lower or hidden stage. The ordinary stage and trap-doors are covered with green baize, or other dark material, so that when the trap-doors are opened, the audience, even those in the gallery, will not readily be able to perceive the opening. The actors

things. The atmosphere may possess great visual clearness, while or objects corresponding with the phantom images which it is desired to represent to the audience, are strongly illuminated by the lime-light, or the electric-light, or other powerful illuminating means may be employed. This light must accompany the actor in any movement he has to make. The hidden stage and the lanterns may be mounted on a carriage on rails, so that when it is necessary for the phantom actor or object on the lower stage to be moved, the lanterns may be caused to move also, or the lanterns may remain stationary whilst the actor moves, provided the whole space through which he moves is sufficiently illuminated. The lanterns are to be provided with means for instantaneously extinguishing or making the light, and for reproducing it, so that the phantom may be made to disappear and re-appear at pleasure, whilst the audience and the stage will be more or less lighted in the ordinary manner, according to the effects desired to be obtained. For this purpose, a board is employed which is capable of being raised into a position so as to entirely cut off the light from the hidden stage when desired, or an ordinary opaque shade attached to the lantern may be used for the purpose, or when using the lime-light the desired effects are caused by gradually or instantaneously (as the case may require) cutting off the supply of gases, and the phantom image may, by any of these means, be caused gradually or instantaneously to fade away. When the trap-doors over the hidden stage are open, the part thereof assists in hiding the lanterns and the opening from the audience. The part acts to screen the lanterns from the audience, and also to ensure that any actor or object on the hidden stage shall not accidentally appear above the level of the visible stage. The phantom actor when standing on the stage leans against the screen, which is inclined so as to be parallel with the glass screen, and is covered with black velvet or other dark material, as is also the stage, in order that no image of either the screen or the stage may be seen in the reflection. The glass screen is a large inclined sheet of plate-glass on the ordinary stage, of sufficient size to reflect the full length of the actors or objects on the hidden stage to the audience in the pit, boxes, and galleries of the theatre. The hidden stage is between the glass and the audience. The glass is mounted in a swing frame so that it may be adjusted to the angle required by screws, or ropes and pulleys, or otherwise. The glass is to be set at such an inclination as to bring the reflected image to the level of the visible stage. This will enable the spectators, whether in the pit, boxes, or gallery, to see the reflected image without any obstruction to the view above the foot-lights, and it will be visible from all parts of the house except those extreme positions which cannot command a view through the glass of that part of the stage where the image is reflected. The glass being in a swing-frame the proper angle of inclination is ascertained experimentally by having persons in different parts of the house to say when the image is shown to them correctly. The scenery is so disposed as to conceal the frame of the glass, and we prefer that the glass should be able to descend into an opening or box beneath the stage, in which case we

counterbalance the glass and frame so that they may easily be raised into the position desired by means of a rope, by which, aided by the bolts, the glass is supported in the required position. In this case the glass may either be adjusted when screened from the audience, and remain in position during the scenes, or (the proper angle of inclination having been previously ascertained by experiment) the glass may be raised on to the ordinary or visible stage and placed in position whilst the scene is before the eyes of the audience, under a subdued light, without the movement being observed, for which latter purpose the top bar of the frame of the glass should be made very light, or be omitted altogether. This arrangement admits of an actor on the visible stage passing across the space which the glass is to occupy, and this he can do just before the appearance of the phantom; and then immediately the glass is run up, the trap-doors are opened, the actor or image on the hidden stage is illuminated, and the phantom appears. This arrangement will render it less likely that the audience should imagine that there is anything interposed between them and the actors, than if the glass plate remained permanently in position during the scene. The hidden or lower stage may be provided with a trunk or well-hole, up which an actor can rise; he will then appear as a spectre rising out of the visible stage. The lanterns may be provided with colored glasses in order to heighten the effect. As the actors on the visible stage do not themselves see the spectral images, marks should be placed on the stage, or other indications made, in order that they may know the position which the spectres appearing to the audience are to occupy. In order to appear upright upon the visible stage, the actor or object on the hidden stage should be inclined so as to be as nearly as practicable parallel with the surface of the glass. In affecting this, assistance is afforded by the screen of the hidden stage. Several sheets of glass may be employed at the same time, if one is not of sufficient width to cover the different parts of the stage at which it is desired that the spectre should appear, the interval or junction being concealed by the introduction of a tree or column, or some other pieces of scenery.—The injunction was granted.

A New Alloy applicable to the manufacture of all metal articles.

From the London Mining Journal, No. 1499.

A new alloy described as applicable to the manufacture of all metal articles, bells, hammers, anvils, rails, and non-cutting tools, has been patented by Mr. M. H. Micolon, of Paris. The alloy consists of iron ings or tin waste, 80 parts of steel, 4 parts of manganese, and 4 parts with manganese or borax. The patentee takes 20 parts of iron turn- of borax; but these proportions may be varied. When it is desired to increase the tenacity of the alloy, 2 or 3 parts of wolfram are added. When the cupola is ready, the iron and steel are poured in, and then the manganese and borax; finally the vessel is filled up with coke; the metal is thus in direct contact with the fuel in the cupola, and by quickly running the fused mass into moulds, bells which possess the sonority of silver, whilst the cost is less than bronze, may be obtained.

The Great Eastern as a Screw Steamer only.

From the London Mining Journal, No. 1494.

The Quarterly Journal of Science contains an article on "Steam Navigation; its Rise, Progress, and Prospects," by Mr. Martin Samuelson, C. E., in which the author suggests that the paddle engines of the *Great Eastern* should be removed; and in order to show the great advantages she would then possess over the other large steamers, he gives the following comparative statement of the capacities of the *Persia* and the *Great Eastern*, under the conditions suggested by him: —"With her paddle engines removed, the *Great Eastern* would carry about 7400 tons of measurement goods, and 12,000 tons of coal (more cargo and less coal in proportion). She would burn about 200 tons of coal per diem, and steam 9 knots per hour. The *Persia* carries 1257 tons of measurement goods, and 1700 tons of coal, and, burning about 150 tons per day, attains an average speed of 12 knots per hour. Thus, if we were to take into consideration the increased speed attained by the *Persia* over the *Great Eastern*, we should have to take the quasi-consumption of the latter, not at 200, but at 260 tons per day. Now, let us compare the work as it would be performed by the two boats, with the coal required by each, and we shall find that the *Persia*, carrying 1257 tons of goods, and consuming 150 tons of coal per day, burns 270 lbs. of coal per day for every ton of goods carried by her. Whilst the *Great Eastern*, carrying 7400 tons of goods, and consuming 268 tons of coal per day, would only burn 81 lbs. of coal per day, for every ton of goods carried. This comparative statement exhibits in a general manner how great is the advantage of a screw over a paddle steamer for trading purposes, but as far as the *Great Eastern* is concerned, we do not hesitate to say that with appropriate internal arrangements she could be made to carry at least 10,000 tons of measurement goods; that with the screw alone and a suitable rig, she would, in an average state of the weather, attain a speed of 10 knots an hour; whilst with a good wind she would keep pace with, if not outstrip, the fastest paddle steamer afloat. A comparison of the trans-Atlantic mail paddle boats, supported by a subsidy, with the screw boats in the same service not so endowed, would further confirm the statement of the superior economy of the screw.

For the Journal of the Franklin Institute.

On Joule's Equivalent of Heat. By JOHN W. NYSTROM, C. E.

In reading an article on "Economy in the Use of Steam," page 313, of this Journal, I came across a passage where the equivalent of power is divided by Joule's equivalent of heat, and the result called "units of heat transmitted into power." This drew my attention to another error in the article on Steam Boiler Explosion, page 328, where I divide units of heat by Joule's equivalent, and call the result horse-power. Both of these results are wrong, and ought to be corrected.

Joule's equivalent of heat is work, or *vis viva*, and not power; it is foot pounds of work, and not foot pounds of power.

Joule's equivalent divided by time will be power. If one equivalent of heat is generated per second, the horse-power will be

$$H = \frac{772}{550} = 1{\cdot}4363 \text{ horses},$$

when 550 lbs. raised one foot per second is one horse-power.

Let us assume the unit for work to be that which one horse-power can accomplish in the time of one hour, which will be about the same as that of a man working one day, and let W represent the number of such units; then the first formula for horse-power, page 328, will be

$$W = \frac{31 \times 180^\circ \times 5{\cdot}5 \times 62{\cdot}5 \times 772}{550 \times 3600} = 747{\cdot}875 \text{ units of work},$$

or it would require 748 men to work one day to do the same amount of damage as that of the explosion.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, May 19, 1864.

William Sellers, President, in the chair.

John H. Towne, Fairman Rogers, } Vice Presidents, } present.

John F. Frazer, Treasurer,

Washington Jones, Recording Secretary.

The minutes of the last meeting were read and approved.

Donations to the Library were received from the Royal Astronomical Society, the Royal Geographical Society, the Statistical Society, and the Society of Arts, London; la Société Industrielle de Mullhouse, France; de Neider-osterreichischen-Gewerbe Vereines, Vienna, Austria; Dr. E. M. Dingler, Augsburg, Germany; Hon. Wm. D. Kelley, and Hon. Geo. Middleton, U. S. Congress, and the Smithsonian Institution, Washington, D. C.; the Young Men's Association, Buffalo, New York; Hon. George Connell, Pennsylvania Senate, Harrisburg, Penna.; the American Philosophical Society, Isaac B. Garrigues, Esq., Prof. John F. Frazer, and Messrs. William Sellers & Co., Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer's report for April was read.

The Board of Managers and Standing Committees reported their minutes.

The Special Committee to revise the By-Laws, reported progress, and at their request the following gentlemen were added to the Committee: Messrs. Frederick Fraley, George Erety, George Harding, John C. Cresson, Samuel V. Merrick, Henry G. Morris, and William Sellers.

The Special Committee to investigate the question of a proper system of screw threads, bolt heads, and nuts, reported progress, and at

their request, Messrs. Robert Briggs and Algernon Roberts were added to the Committee.

The Special Committee on the new Charter, reported the Charter as passed by the Legislature, which was read, and, on motion, accepted.

The Special Committee on steam expansion, reported progress.

On motion, it was

Resolved, That when the Institute adjourn, they adjourn to meet on Thursday evening, June 9, proximo, to receive the report of the Committee on the revision of the By-Laws, and consider the same.

One resignation of membership in the Institute, was read and accepted.

Candidates for membership in the Institute (13) were proposed, and the candidates proposed at the last meeting (7) were duly elected.

Mr. J. B. Britton exhibited a model of his patented Furnace for Roasting Ores, etc.

In this furnace the ore is contained in a central vertical chamber, across which the products of combustion from the fire-place are caused to pass regularly in one direction. By a discharging device the rate of descent of the ore through the chamber can be regulated at pleasure, so that each particle of ore can be submitted to the action of the fire for any length of time desired. By carrying the draft *across* the chamber the products of combustion are brought in direct contact with every portion of the ore, and the sulphurous and other deleterious vapors are taken direct to the chimney, and are not carried among the particles of unroasted ore, as in ordinary kilns.

A model of Harthol & Lee's "American Farm Gate" was exhibited. In this gate the bars are jointed together at the point of their intersection, and all the horizontal bars are jointed at one end to the side post, so that when these bars are raised to a vertical position the others will be brought close together, leaving the passage way between the posts clear.

Barry, Waples & Singleton's Bilge Water Gauge, for measuring the depth of water in the holds of vessels, etc., was exhibited.

This gauge consists of an inverted siphon containing mercury, the short leg of the siphon being about four times the diameter of the other, and having connected to it one end of a copper pipe, the other end of which communicates with an air chamber in the hold. The air chamber is a pipe about 14 inches long and one inch inside diameter, open at the bottom and closed at the top. When the water rises in the hold, the air in the air chamber is compressed, and this pressure is communicated to the mercury in the siphon, which, by its height, indicates the depth of water in the hold.

This gauge is also admirably adapted for showing the depth of water in a screw or sectional dock, reservoir, or cistern, there being no practical limit to its capacity, as it will show as accurately the depth of fifty feet as one foot.

Mr. James D. Rice submitted for examination several forms of Lightning Rod Points, and mode the following remarks:

A new invention is not claimed for any of the lightning rod points exhibited, but simply a modification of a point constructed by Professor Robert T. Hare.

Through the kindness of Judge Hare, I have obtained this point for exhibition this evening: at a future time it is to be presented to the Franklin Institute to be deposited in their model-room. This point is constructed of 18 small copper wires, soldered to the upper end of an iron rod, the junction surrounded by a zinc ball; the object of which is to preserve the copper points from oxidation. This point was made over 30 years ago—as I find it described in Professor Hare's Chemistry—and, as may be seen by examination, the copper wires are in good order.

I exhibit two points of my *own* construction, differing somewhat from Professor Hare's but upon the same principle. One of these was placed upon a flag-pole upon the American Mechanics' Hall, and was struck by lightning last summer; one of the copper points shows slight marks of fusion, but not injured for further use; no other effects could be traced upon the rod or building. Here is another point which has been struck, a platina point, of M'Allister's manufacture; the platina is all melted off, and the rest of the points much torn by the explosion. This I have found to be the case in all instances with the platina point—doubtless owing to its very *low conducting power* and the *form* in which it is used. Another objection to this point is the very imperfect method of connecting the point to the rod. I consider the platina points of little value for lightning rods. Points should be so constructed as to be uninjured when repeatedly struck.

In replying to the remarks of Professor Frazer, my opinion is, that although lightning conductors in complete order *do* silently convey electricity from a cloud, yet they cannot in this way prevent a stroke at all times; if they could, lightning should never strike in this city where there are many hundred rods, many of which are as complete as possible, and yet it does strike from 3 to 6 times or more every season. Again, whoever has watched the lightnings flash during storms, has observed it to pass horizontally a long distance—probably many miles—and then descend to the earth; in such cases the clouds being too far off to be discharged silently by the conductor; it then becomes the office of the lightning rod to receive and convey the stroke harmlessly into the earth. In an *instant* it may pass from the clouds, say 10 or 15 miles distant, to one directly over us, and thence to the earth, destroying life and property without descrimination, unless protected by suitable conductors.

A word or two relative to insulation of rods; no good authority sanctions their use. Professor Hare's rod erected by himself, on the contrary, is soldered to the metal roof, making the roof a part of the conductor. This principle I have adopted on many large buildings in this city, such as railroad depots, market-houses, Girard College buildings, &c.

A Comparison of some of the Meteorological Phenomena of MAY, 1864, *with those of* MAY, 1863, *and of the same month for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By JAMES A. KIRKPATRICK, A. M.

	May, 1864.	May, 1863.	May, for 13 years.
Thermometer—Highest—degree, .	86·00°	90·00°	90·00°
" " date, .	10th & 31st.	23d.	7th'60;23d'63
" Warmest day—Mean,	78·17	27·84	79·83
" " " date,	9th.	23d.	23d, 1863.
" Lowest—degree, .	41·00	40·00	35·00
" " date, .	3d.	7th.	7th, 1854.
" Coldest day—Mean,	49·67	42·50	40·00
" " " date, .	3d.	6th.	3d, 1861.
" Mean daily oscillation,	16·61	19·15	17·24
" " " range, .	5·03	5·43	5·50
" Means at 7 A.M., .	62·53	59·32	58·57
" " 2 P.M., .	73·35	71·76	69·93
" " 9 P.M., .	65·15	62·64	61·58
" " for the month,	67·01	64·57	63·36
Barometer—Highest—Inches, .	30,000 in.	29·975 in.	30·338 in.
" " date, . .	5th.	21st.	4th, 1852.
" Greatest mean daily press.	29·978	29·962	30·273
" " date, . .	5th.	21st.	5th, 1852.
" Lowest—Inches, .	29·379	29·294	29·096
" " date, . .	10th.	31st.	27th, 1861.
" Least mean daily press.,	29·437	29·343	29·243
" " date, . .	10th.	31st.	27th, 1861.
" Mean daily range, .	0·108	0·085	0·120
" Means at 7 A.M., .	29·663	29·783	29·812
" " 2 P.M., .	29·627	29·736	29·775
" " 9 P.M., .	29·665	29·769	29·779
" " for the month, .	29·652	29·763	29·795
Force of Vapor—Greatest—Inches,	0·714 in.	0·070 in.	0·771 in.
" " " date, .	24th.	30th.	14th, 1854.
" " Least—Inches, .	·202	·189	·069
" " " date, .	3d.	5th.	2d, 1861.
" " Means at 7 A.M., .	·423	·375	·357
" " " 2 P.M., .	·452	·365	·375
" " " 9 P.M., .	·461	·406	·380
" " " for the month,	·445	·382	·371
Relative Humidity—Greatest—per ct.	97 per ct.	91 per ct.	100·0 per ct.
" " " date,	26th.	7th.	Often.
" " Least—per ct.,	32·0	28·0	16·0
" " " date, .	28th.	22d.	5th, 1855.
" " Means at 7 A.M.,	73·2	72·3	71·4
" " " 2 P.M.,	54·5	48·3	51·2
" " " 9 P.M.,	73·6	69·8	68·5
" " "for the month	67·1	63·5	63·7
Clouds—Number of clear days,* .	7	15	10·4
" " cloudy days, .	24	16	20·6
" Means of sky cov'd at 7 A.M.,	62·9 per ct.	51·0 per ct.	58·3 per ct.
" " " " 2 P.M.,	69·0	55·5	60·3
" " " " 9 P.M.,	58·4	38·1	46·5
" " " for the month,	63·4	48·2	55·0
Rain Amount, .	9·042 in.	4·792 in.	4·609 in.
No. of days on which Rain or Snow fell,	12	11	12·7
Prevailing Winds—Times in 1000	S 22°23′W·105	N 77°8′W·877	N74°38′W·105

* Less than one-third covered at the hours of observation.

A Comparison of some of the Meteorological Phenomena of the SPRING *of* 1864, *with that of* 1863, *and of the same Season for* THIRTEEN *years, at Philadelphia, Pa.* Barometer 60 feet above mean tide in the Delaware River. Latitude 39° 57½′ N.; Longitude 75° 10½′ W. from Greenwich. By J. A. KIRKPATRICK, A. M.

	Spring, 1864.	Spring, 1863.	Spring, for 13 years.
Thermometer—Highest—degree, .	86·00°	90·00°	90·00°
" " date, .	May 10 & 31,	May 23d.	My 7'60; My 23 '63
" Warmest day—Mean,	78·19	79·83	78·83
" " " date,	May 9,	May 23d.	May 23, 1860.
" Lowest—degree, .	20·00	15·00	4·00
" " date, . .	March 22d.	March 5th.	March 10 '56.
" Coldest day—Mean,	27·33	24·17	11·50
" " " date, .	March 22d.	March 5th.	March 10 '56.
" Mean daily oscillation,	15·19	15·95	16·20
" " " range, .	5·14	5·77	5·98
" Means at 7 A. M., .	47·26	45·19	46·47
" " 2 P. M., .	58·00	55·68	57·86
" " 9 P. M., .	50·88	48·62	50·30
" " for the Spring,	52·05	49·83	51·54
Barometer—Highest—Inches, .	30·131 in.	30·384 in.	30·522 in.
" " date, . .	March 3d.	March 21st.	March 3d, '52.
" Greatest mean daily press.	30·090	30·311	30·458
" " date, . .	March 3d.	March 20th.	April 3d, '54.
" Lowest—Inches, . .	29·184	29·260	28·884
" " date, . .	March 30.	April 2d.	Ap. 21st. '52.
" Least mean daily press.,	29·246	29·343	28·959
" " date, . .	March 30.	May 31st.	Ap. 21st, '52.
" Mean daily range, .	0·135	0·154	0·161
" Means at 7 A. M., .	29·705	29·818	29·822
" " 2 P. M., .	29·661	29·772	29·777
" " 9 P. M., .	29·706	29·819	29·808
" " for the Spring,	29·691	29·803	49·802
Force of Vapor—Greatest—Inches,	0·714 in.	0·700 in.	0·771 in.
" " " date, .	May 24th.	May 30th.	May 14, 1854.
" " Least—Inches, .	0·664	·050	·023
" " " date, .	March 20,	March 15th.	Mar. 5, 1858.
" " Means at 7 A. M.,	·263	·243	·249
" " " 2 P. M.,	·278	·243	·265
" " " 9 P. M.,	·286	·264	·269
" " " for the Spring,	·276	·250	·261
Relative Humidity—Greatest—per ct.,	97·0 per ct.	96·0 per ct.	100·0 per ct.
" " " date,	May 26th.	April 24th.	Often.
" " Least—per ct.,	18·0	15.0	13·0
" " " date, .	Mar. 9; Ap. 7.	April 24th.	April 13, '52.
" " Means at 7 A.M.,	71·9	72·3	72·2
" " " 2 P.M.,	52·4	51·7	51·8
" " " 9 P.M.,	69·1	69·4	67·9
" " " for Spring,	64·5	64·5	64·0
Clouds—Number of clear days,* .	21	27	28·8
" " cloudy days, .	71	65	63·2
" Means of sky cov'd at 7 A.M.	52·2 per ct.	58·7 per ct.	60·3 per ct.
" " " " 2 P.M.	68·6	67·7	62·6
" " " " 9 P.M.	59·2	50·4	48·6
" " " for Spring,	63·3	58·9	57·2
Rain and Melted Snow—Amount,	18·763 in.	18·465 in.	12·816 in.
No. of days on which Rain or Snow fell,	36·	43·	36·5
Prevailing Winds—Times in 1000,	N52°39′W·096	N40°15′W·170	N67°58′W·186

* Less than one-third covered at the hours of observation.

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